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# STUDIES ON THE ORGANIZATION OF THE GIANT GLAND CHROMOSOMES OF DIPTERA

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IT HAS been evident since the recent discovery of their true significance (Heitz and Bauer 1933, Painter 1933) that the giant salivary gland chromosomes of Diptera offer an exceptional opportunity for detailed studies on chromosome structure, particularly in relation to genes. The importance of the problems and the favorability of the material have attracted the attention of numerous investigators, with the result that various hypotheses or interpretations are now current as to the organization of these chromosomes. These interpretations are of four distinctly different types—a fact which serves both to indicate the difficulty of interpreting the evidence and to suggest the need of caution in accepting any of the hypotheses. Since some of the interpretations are being used, nevertheless, as a basis for considerations on such topics as the nature of genes (Muller and Prokofieva 1935, Wrinch 1935) it seems desirable to subject both the evidence and the interpretations to critical examination as they are presented.

One purpose of the present paper is to do this. More specifically, the purpose is to examine the nature of the evidence which has led to the view that the giant chromosome is composed of numerous discrete threads or chromonemata *which are visible as such* in the preparations, and that the chromatic "granules" in the chromosome represent individual genes or chromomeres. Since this paper was sent to press it has been possible to include some brief comments on recent papers and some evidence derived from *Chironomus* by one of us (Metz).

The most conspicuous characteristic of the giant chromosome is its relatively enormous size. The next most conspicuous feature is the organization into transverse chromatic discs or "bands" separated from one another by achromatic interspaces (Fig. 1).

Since the immediate problem concerning these chromosomes is that of interpreting them and their visible constituents in terms of "ordinary" chromosomes and their constituents, the question imme-





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diately arises as to how the giant chromosomes attain their great size and what the chromatic discs represent. The size may be accounted for through growth alone, or through a combination of growth and multiplication ("division") of component parts. Particular attention will be given to these possibilities. The series of chromatic discs immediately suggests the series of "chromioles" or "chromomeres" in the uncoiled chromonema of an ordinary chromosome; but here again there is an enormous difference in size and the same possibilities of growth or multiplication suggest themselves as alternative explanations of the difference.

In considering the latter problem attention has been given by all investigators to the fact that many of the discs are granular in nature and may look like transverse rows of granules when the chromosome is viewed from the side. This characteristic has led to the suggestion (Koltzoff 1934, Bridges 1935) that each disc is compound and that each granule represents a gene or chromomere. On this view the chromosome is considered as having arisen through multiplication of the original chromonema and its constituent parts (genes, chromomeres) and the number of divisions of the chromonema is supposed to be represented by

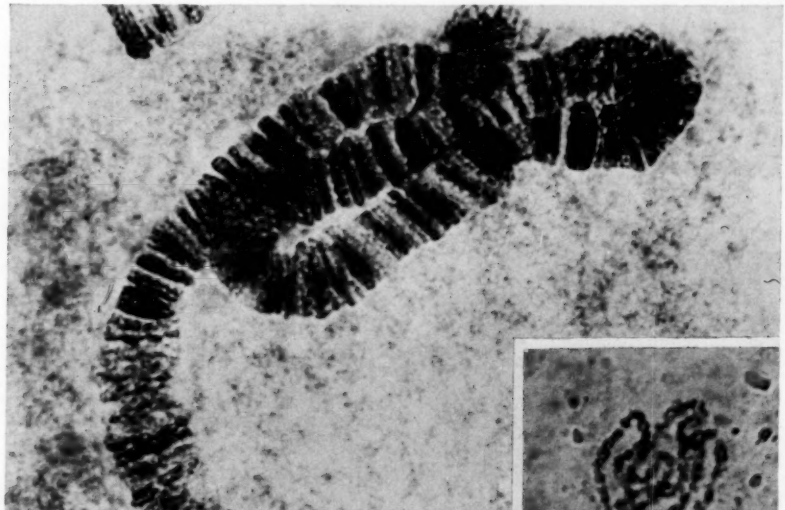



FIG. 1. PHOTOGRAPH OF THE X CHROMOSOME (PAIR) IN A SALIVARY GLAND CELL OF SCIARA OCELLARIS COMST.
From an aceto-carmine preparation. ×1710. If this chromosome were derived by simple multiplication of visible chromonemata it should look like those shown in Fig. 2.

FIG. 2. PHOTOGRAPH OF A MULTIPLE CHROMOSOME COMPLEX IN CULEX PIPIENS, (BY MR. C. A. BERGER)
Prophase, from an aceto-carmine preparation. Magnification same as Fig. 1. The group includes three aggregates, corresponding to the three V-shaped pairs of chromosomes of this species. Each aggregate represents 8 (or 16?) closely associated chromonemata derived by multiplication of the original chromonemata of the pair. ×1710.

the number of granules in a disc. It is stated or implied on this view that the chromonema and its constituents possess definite size limits and that growth automatically results in duplication.

Thus the giant chromosome is regarded, on this view, as a cable made up of thread-like chromonemata on which the granules are arranged like beads. Two types of interpretation have been made on this basis, as will be noted later, but they need not be distinguished at this point. Since the essential feature of each is apparently the assumption that the constituents under consideration have definite size limits, and that the constituents visible in the giant chromosome are comparable to those in the chromonema of an ordinary cell (Koltzoff 1934, Bauer 1935), attention may first be given to the question of size relationships.

In considering these it should be recalled that in the gland cells under consideration the chromosomes are associated intimately in pairs, so what is called a "chromosome" is ordinarily a pair of homologues fused side by side. It should also be recalled that the giant chromosome is not comparable in condition to the condensed metaphase chromosome of an ordinary cell, but to the elongated chromosome or chromonema of the interphase or early prophase.

### SIZE RELATIONSHIPS

In *Sciara*, as shown previously (Metz 1935a) the size relations between an ordinary chromosome of an embryonic cell and one of the giant chromosomes of a salivary gland cell are apparently on the order of 1:1000 or more in volume. It has not been possible to make exact measurements; but the following observations on *Sciara ocellaris* Comst. serve to indicate roughly the comparative sizes of the structures under consideration.

Our observations indicate that the diameter of a single giant chromosome (pair) may be greater than the diameter of an entire resting nucleus of an ordinary embryonic cell in the same larva. The volume of such a chromosome appears to be more than 100 times that of such a nucleus. A single chromonema in an ordinary embryonic cell is too minute and delicate to measure, but since its bulk is much less than that of the metaphase chromosome, which contains a considerable amount of "matrix" material, and probably two chromonemata, it seems probable that a giant chromosome is more than 1000 times as large as the chromonema, in volume.

If, therefore, the giant chromosome represents a bundle or cable of ordinary chromonemata it would be expected that the number of chromonemata would approximate that just mentioned. Since the number would be indicated by the number of granules in a disc, on the view under consideration, the granules should be present in corresponding numbers. In *Sciara*, however, the number of granules ranges from 5 or 6 up to possibly 30 or 40.

Similarly, great discrepancies are met when consideration is given to the size of the granules and to the length of the giant chromosomes. According to our observations the size of many of the granules in the giant chromosomes is many times as large as that of anything which could be considered a unit chromomere in the chromonema of an ordinary cell. Likewise, the length of the giant chromosome appears to be at least several times that of an ordinary chromonema. On the view under consideration a chromonema should include one granule from each disc, together with intervening achromatic material. It is obvious, however, that in volume the latter exceeds the former many times. It seems safe to say that in some cases a single granule exceeds the entire ordinary chromonema in bulk, yet the number of granules in a hypothetical chromonema in a giant chromosome, as represented by the number of discs in such a chromosome, is expressed in hundreds.

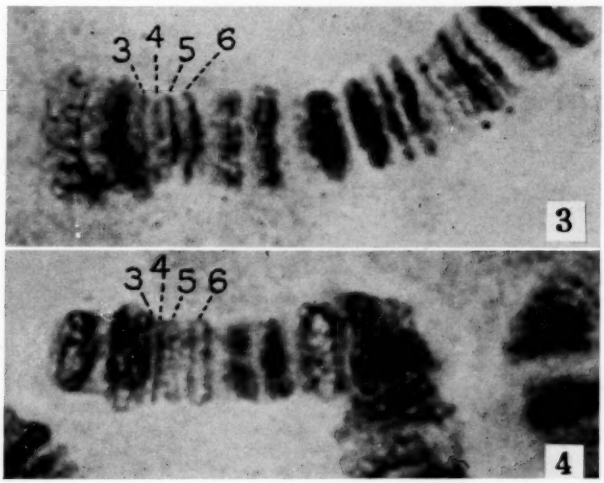
As noted previously (Metz 1937 and earlier papers) such considerations indicate either that the components of a chromonema do not have fixed size limits corresponding to those seen in an ordinary cell, or else that the visible structures under consideration (granules) are not chromomeres representing individual units.

### COMPARISON WITH AN ACTUAL CASE OF COMPOUND CHROMONEMATA

Another line of evidence is even more significant in this connection. This is provided by the multiple chromosome complexes in the iliac cells of *Culex* larvae, as shown by Mr. C. A. Berger (1936, 1937 and

unpublished) in this laboratory. Here the chromosomes undergo repeated divisions while in the resting stage, and produce compound structures which in early prophase represent just what is postulated on the view considered above. The four, eight or sixteen chromonemata are associated side by side in the form of a cable in prophase, and later separate to make definite chromosomes which go through mitosis.

In Fig. 2 is shown a photograph of such a prophase figure, kindly provided by Mr. Berger. Each apparent chromosome in this figure represents 8 (possibly 16) chromonemata intimately associated side by side. For comparison a giant chromosome (the X) in *Sciara ocellaris* is shown at the same magnification in




FIGS. 3 AND 4. PHOTOGRAPHS OF END 1 OF CHROMOSOME A OF SCIARA OCELLARIS (VAR. B), FROM DIFFERENT CELL OF ONE SALIVARY GLAND, TO SHOW VARIABILITY IN APPEARANCE OF INDIVIDUAL DISCS

From an aceto-carmine preparation. ×3000. Compare especially disc 6, which appears as a single band or row of granules in Fig. 3 and as two bands or rows of granules in Fig. 4 (see Metz, 1937). The numbering of the discs here corresponds to that in previous figures of this chromosome (Metz, 1935a, Fig. 5B, 5D).

Fig. 1. The ordinary chromosomes in *Culex* are larger than those of *Sciara*, so the contrast is not due to a basic difference in size in favor of the *Sciara* chromosomes.

Three features call for special comment in connection with this comparison.

(1) If the giant chromosome of *Sciara* were made up of 10 to 20 chromonemata (which would be the maximum number expected if each granule of a disc identifies a chromonema because most discs do not have more than that many granules) such a chromosome should be approximately twice the size of one of the composite structures in *Culex*. Actually, however, it is hundreds of times as large.

(2) On the hypothesis under consideration one chromonema in a *Culex* composite chromosome (i.e. one eighth of one of the composites in Fig. 2) should be equivalent in size to a structure made up of one granule from each of the discs in the giant chromosome, plus connecting achromatic material. It is obvious, however, that no such relation exists. The latter structure would be many times as large as the single chromonema. The same conclusion is reached if a giant chromosome of *Drosophila* or *Chironomus*, rather than of *Sciara*, is used for comparison, although conditions differ somewhat in detail in the different genera. In *Chironomus*, for example, there is a finer and more delicate internal organization, with smaller and more numerous granules in the discs.

(3) If the giant chromosome is a multiple of the type under consideration it should be approximately the length of an ordinary chromonema, which would presumably be not greatly different from that of one of the early prophase composites of a comparable chromosome in *Culex*. In the *Culex* group (Fig. 2) there are three composites, each representing a pair of relatively long, V-shaped chromosomes multiplied. Each such chromosome is comparable to the V-shaped chromosome in *Sciara ocellaris*, which in the giant cells is approximately twice the length of the chromosome shown in Fig. 1. Comparison on this basis indicates that the giant chromosome is from seven to ten times as long as the comparable prophase composite in *Culex*.

It has not been possible to demonstrate that an ordinary fully extended chromonema in *Sciara* or *Culex* is not seven to ten times as long as it appears in prophase, for in the resting stage direct measurements cannot be made. Apparently, however, in *Culex* the earliest prophase composites which are suitable for study contain extended chromonemata, and these are not greatly different in length from those shown in Fig. 2. It seems probable, therefore, that an extended chromonema is not more than twice the length of these composites, or more than one fifth to one third the length of a comparable giant chromosome.

This fact in turn makes it probable, as inferred from other evidence also, that the structure which gave rise to the giant chromosome has undergone a great increase in length, through growth, irrespective of how the increase in diameter has been brought about. If that is true, the size limits of the ordinary chromonema are not fixed or even approximately fixed as would be expected on the hypothesis under consideration. If such an increase in length can be effected there seems no obvious reason why a corresponding increase in diameter cannot be brought about by growth without division.

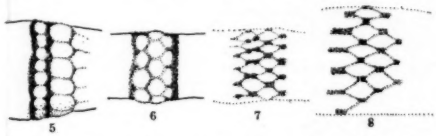
If objection is made to this interpretation, and an ordinary chromonema is considered equal in length to a giant chromosome, serious difficulties of the type considered under item (2) are encountered, for this would require such attenuation as to make the chromonema practically invisible. The result may be visualized by considering one eighth or one sixteenth of one of the composites shown in Fig. 2, extended to twice the length of the giant chromosome shown in Fig. 1.

In the light of such evidence we can only conclude that as it stands the hypothesis under consideration is inapplicable to the giant chromosomes. It may next be inquired whether or not any modification of it is applicable. As originally put forward by Koltzoff and by Bridges it assumed that the chromonemata were, in *Drosophila*, usually 16 or 32 in number and arranged spirally around the periphery of the chromosome (pair), which was said to have a central axis. This view appears definitely to be ruled out by the findings of Metz and Gay (1934a, b), Koller (1935), Bauer (1935, 1936) and Metz (1935a, b) that no differentiated axis is present and that the general structure is uniform through the chromosome transversely at any one level (Fig. 16). The conception of visible multiple structure, however, is accepted by Koller and by Bauer, who consider the giant chromosome a uniform bundle or cable with the chromonemata extending essentially straight, instead of a hollow structure with threads extending spirally. It is interesting, in this connection, to note that this latter interpretation is based largely on study of the same material (*Drosophila* and *Chironomus*) used by Koltzoff and by Bridges in arriving at the former interpretation. This serves to show how difficult it is to interpret the evidence.

As intimated above, the visible structures in the giant chromosomes all appear to be much too large to represent unit parts of ordinary chromonemata. They can only be interpreted on this basis if it is assumed that the original units have undergone enlargement far beyond the limits reached in ordinary embryonic cells. If this assumption is made, and the original hypothesis modified accordingly, the giant chromosome might be looked upon as a structure in which the ordinary relationships between growth and division have been upset, and either the original chromonema undergoes a few divisions, followed by excessive growth without division, or else the chromonemata and their constituents divide only after they reach a much greater size than usual. Since such a view (apparently held by Painter and Griffen,

1936) abandons the conception of size limits, however, it has little theoretical advantage over the assumption that the enlargement is a result of growth alone, for the difference is only one of degree (see Metz 1937). Before examining the evidence for and against the assumption a third alternative may be considered.

It may be suggested that the assumption underlying the first mentioned hypothesis is correct and that the giant chromosome is a multiple in which the individual chromonemata are so numerous and small as to be invisible individually, but that they are associated in groups or clusters which are visible. On a view of this type the granules would be compound, each made up of numerous invisible units (genes or chromomeres). Each granule could be considered a definite multiple, and hence a unit of higher order, or it could be considered as of no individual significance.




FIGS. 5–8. FREE HAND DRAWINGS OF SELECTED REGIONS IN SALIVARY GLAND CHROMOSOMES OF SCIARA OCELLARIS COMST.

5, 6, 8, from aceto-carmine preparations; 7, from an osmic-aceto-carmine preparation. Only one optical level is shown in each case.

5, an example illustrating the relationship between the achromatic droplets and the surrounding chromatin—the latter confined mainly to the discs. Also illustrates difference in number of droplets at different loci. 6, example of a region in which the chromatic material is more evenly distributed around the droplets, giving a definitely honeycomb type of structure. (Compare with region d in photograph in Fig. 13, and with diagram in Fig. 10.) 7 and 8, moderately stretched regions of the type in which the chromatic material is mostly localized in block-like granules connected by delicate lines, giving the appearance of a network. The evidence indicates that in such cases each granule is made up of material from two discs, as may be seen by comparing with Fig. 6.

## THE NATURE OF THE "GRANULES"

These considerations serve to focus attention directly on the individual granules and on any structures which might represent chromonemata, either single or compound. If the granules represent chromomeres, and serve to identify chromonemata, in either sense, it would be expected that the number would be uniform in the discs of one chromosome. But, as pointed out previously (see especially Metz 1935b), there is a wide difference in this respect in our material. An example of such variation is shown in the accompanying Fig. 20. It might be maintained that large granules in small number represent higher but definite multiples, but we have been unable to find any evidence for such a view. In the case of some discs a small number of small granules is found.

It would similarly be expected on the view under consideration that the number of granules would be multiples of two; whereas, as indicated previously the number appears to be variable and not consistent in this respect. Of greater significance than either of these lines of evidence, however, are two others. One is the great variability in the size, number and general characteristics of the granules of a given disc in different cells, even in comparable chromosomes of the same gland. This feature has been considered somewhat in an earlier paper (Metz 1935b) and will receive further treatment elsewhere. The other line of evidence is that which shows that the materials of a "disc" may appear in the form of a single row of granules in one cell and two rows of granules in another, even in the same gland. An illustration of such a case is shown in Figs. 3 and 4. These are photographs of end 1 of chromosome A in *Sciara ocellaris*, both from the same gland. In the former, "discs" numbered 3, 4, 5 and 6 all appear single—i.e. each looks like a band or a single row of granules. In the latter (Fig. 4), however, 5 and 6 both appear double; each is represented by two rows of granules. This is especially obvious in the case of disc 6, although under the microscope it is clear also in that of disc 5. The difference between the conditions in the two chromosomes is apparently due to a different distribution of chromatic and achromatic materials in the two, as described elsewhere (Metz 1937). An accumulation of achromatic substance within the material of the "disc" gives it an inflated appearance, and under this condition the two faces look like separate discs, each seen as a row of granules in edge

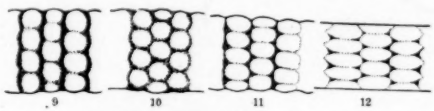
view. The evidence is strong that a rearrangement of chromatic material is involved here, and that the granules in the one case bear no relation to those in the other. (It is, of course, not certain that any "disc" here is really single.)

This leads to consideration of another aspect of structure and to the question of normal variability. These can only be treated briefly at this time, but the general features may be summarized as follows. For the most part this summary represents an outline of the interpretation advanced in previous papers by Metz and Gay and by Metz (cited above).

due to injury or poor fixation; but the evidence now indicates that both conditions are normal and may even be reversible (Metz 1937).

4. In addition to the variation in appearance due to differences in dispersion of chromatic material there is considerable variation due apparently to differences in compression or tension within the chromosome. If the achromatic droplets are not tightly pressed together they appear rounded as indicated in Figs. 5 (at left) 9, 10, 18, and at *a* in 14 and 17). If, however, they are tightly pressed together, or slightly stretched, their interfaces become flattened and their appearance hexagonal as indicated in Figs. 5, 6, 11, 12, and 13 (at *d*).

5. The general structure, then, is essentially alveolar or honeycomb-like, modified by the stratification due to the discs (see papers of Metz and Gay,




FIGS. 9–12. DIAGRAMS ILLUSTRATING VARIOUS TYPES OF DISTRIBUTION OF CHROMATIC MATERIAL AROUND THE ACHROMATIC DROPLETS

In 9 the chromatin is mainly confined to the discs, with relatively little extending from one disc to the next between the droplets. In 10 more of the chromatin extends between the droplets, giving a honeycomb pattern. With still further accumulation between the droplets, and less in the discs, the pattern shown in Figs. 7 and 8 appears. In 11 and 12 the chromatin is confined largely to the regions of the discs, but tends to form granules in the interstices between the droplets. The discs or transverse rows of granules may be zig-zag, as shown here, or in the absence of longitudinal tension may be straight.

## RELATIONSHIP BETWEEN CHROMATIC AND ACHROMATIC MATERIALS

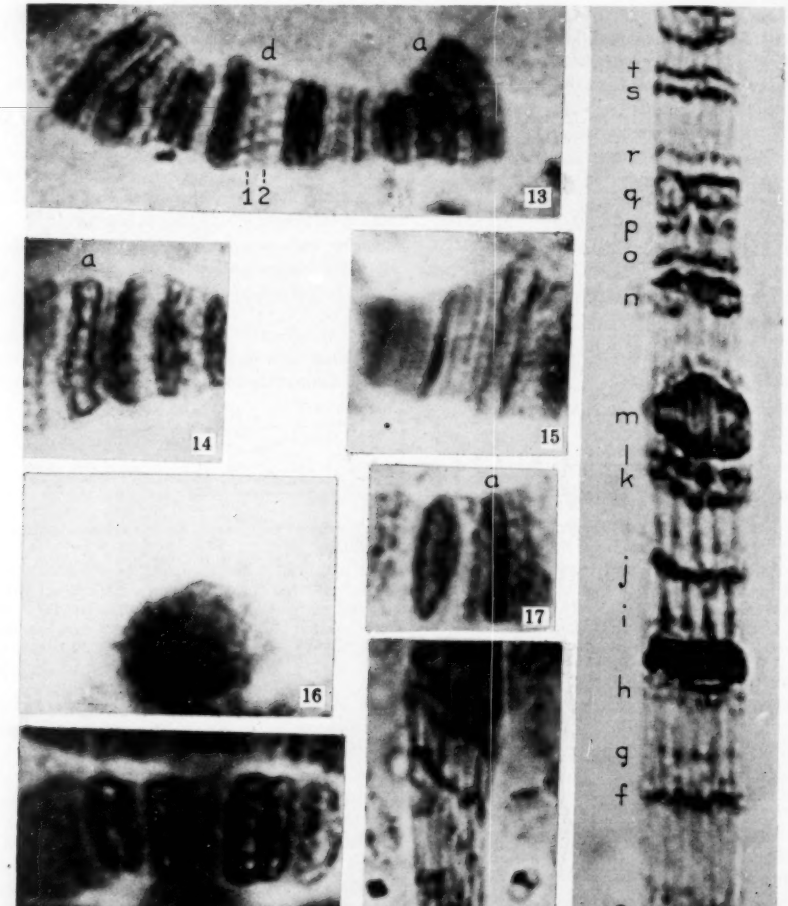
1. In all the forms we have studied (*Sciara*, *Chironomus*, *Drosophila*) it seems clear that the achromatic material and the chromatic material in the giant chromosome are organized differently. The former is in the form of droplets, at least in the fixed chromosome, and hence is in this sense discontinuous. The latter, on the other hand, does not appear to be in this form, but rather to be essentially continuous.

2. Each two successive chromatic discs are separated by a layer or plate of achromatic droplets as indicated in Figs. 5, 6, 9–12, 13, 14, 17 and 18.

3. Although confined largely to the discs, the chromatic material also extends between and around the achromatic droplets separating one disc from the next. The degree of dispersion of the chromatic material varies widely. In some cases the discs are sharp and clear cut, with relatively little chromatic material around the intervening droplets (Fig. 15); in others much of the chromatic material is in the latter position (Figs. 5, 6, 14, 17, 18). At first it was thought that the sharply "banded" condition was normal and the condition of wide dispersal of chromatin, or "vesiculated" condition, was abnormal and and of Metz cited above). In many cases such organization is clearly visible and may be photographed (Figs. 13, 14, 17, 18).

6. The numbers and sizes of the achromatic droplets differ in different regions—presumably reflecting qualitative differences in the materials (Fig. 5). Also there is considerable variation in a given region of a given chromosome in different cells, even when the chromosomes are of comparable size and in the same gland. Each region, however, exhibits its own characteristic tendencies. The variation is not purely a random one.

7. Since a plate of achromatic droplets separates two adjacent chromatic discs, the interfaces between the compressed droplets, together with more or less chromatic material, serve to form short longitudinal lines connecting the two discs, as indicated in Figs. 5, 11, 12, 13 (at *d*) 14, 17 and 18. These lines may be very conspicuous or very faint, depending on the amount of chromatic material present, as indicated above. In the "vesiculated" condition (Fig. 18) they are relatively conspicuous. It is especially significant that in the undistorted chromosome these short lines, for the most part, are not themselves aligned to give the appearance of continuous threads,




FIGS. 13–20. PHOTOGRAPHS OF SELECTED REGIONS IN SALIVARY GLAND CHROMOSOMES OF SCIARA OCELLARIS COMST., EXCEPT 18 AND 19 WHICH ARE FROM SCIARA COPROPHILA LINT.

All are from smear preparations. Details are discussed in the text. Photographs made with 3 mm. oil immersion objective, 1.4 aperture, and 10× or 15× ocular. Original magnification in most cases ×1500. Present magnification ×3000 except in the case of Figs. 15, 17 and 18, as noted below. 13, right end of chro-

but are staggered. Those between two discs alternate in position with those between the next two discs as indicated in the figures just cited.

8. When chromatic granules are conspicuous in a disc they lie at the ends of these lines or interfaces. That is, they lie at the points in the discs where three or more adjacent achromatic droplets come together, as indicated in Figs. 11 and 12 and in suitable regions in the photographs of Figs. 13, 14 and 17. In a descriptive sense, at least, they represent thickenings in the discs at the points where there is a maximum amount of space between adjacent droplets, including those on both sides of the disc.

9. The number of granules in a disc is correlated with, and apparently determined by, the number of achromatic droplets adjacent to it. Similarly, the size of the granules appears to be determined by the size and number of the droplets, together with the amount of chromatic material in the disc; and the shape of the granules appears to depend on these same factors, together with the position of the droplets with respect to one another.

10. In some cases chromatic granules appear to be formed in a somewhat different manner, as indicated in an earlier paper (Metz 1935b). Here the chromatic material is located primarily in the position of the longitudinal lines or interfaces between the discs rather than in the discs themselves. The method of formation of such granules may be understood by comparing Figs. 21 (at 1, 2) 13 (at 1, 2) and 8; or 9, 10 and 8. In 21 and 9 the chromatin is mainly in the discs; in 13 and 10 it extends between the droplets separating adjacent discs, while in 8 (also 7) it lies mainly in the latter position so that the discs look like zig-zag lines which appear to join at intervals to form "granules."

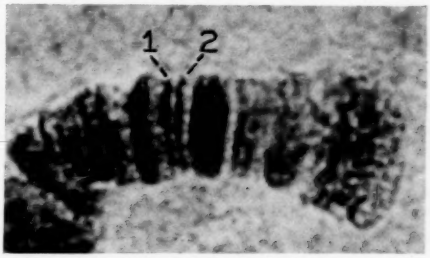



FIG. 21. PHOTOGRAPH OF RIGHT END OF CHROMOSOME A IN SCIARA OCELLARIS, FROM AN ACETO-CARMINE PREPARATION. ×3000

For comparison with Fig. 13, which shows the same region in another preparation. Compare especially discs marked 1 and 2 with the corresponding discs, similarly designated, in Fig. 13. In the present figure the chromatin is shown concentrated in the discs, which look like granular bands; very little extends between the droplets separating the discs. In the other case (Fig. 13) the chromatin is more evenly distributed around the droplets, revealing the honeycomb-like organization in which the two discs appear as zig-zag bands.

mosome A from an aceto-carmine preparation, showing the honeycomb structure at d. (Compare with Fig. 21.) Similar type of structure is shown indistinctly near the tip of the chromosome, at a. (Compare with Fig. 17, which shows this same tip region in another preparation.) 14, from an aceto-carmine preparation. Under the microscope the vacuolated region between the two dark bands at a shows a structure similar to that represented in Figs. 5 and 9. 15, small portion of a chromosome in a formalin aceto-carmine preparation showing the chromatin confined mainly to the discs, forming relatively sharp "bands". ×2600. 16, optical section of a chromosome (pair) in an osmic-formalin-aceto-carmine preparation, showing that the granules are not limited to the periphery of the chromosome but that the structure is essentially uniform throughout the cross section. 17, extreme tip region at right end of chromosome A, showing conditions similar to those represented in Fig. 5 and the diagrams of Figs. 9 and 10. From an aceto-carmine preparation. ×3250. 18, two selected regions from one chromosome group in a preparation treated with 2 per cent chromic acid followed by aceto-carmine, showing the conspicuous alveolar or vacuolated condition. Under the microscope the structure appears more definitely like that shown diagramatically in Figs. 9 and 10. The chromatic material extends between and around the droplets or alveoli instead of being limited mainly to the discs. Similar conditions are often found in ordinary aceto-carmine preparations. ×3270. 19, stretched region of a chromosome from the same preparation as Fig. 18, showing how the chromatic walls or interfaces between the droplets become pulled out so that in optical section they give the appearance of threadlike striations. 20, portion of chromosome A severely stretched. Aceto-carmine preparation. The photograph represents one optical level at or near the upper surface of the chromosome. Note the wide differences in number and appearance of the striations in different regions, the fact that these striations seldom form continuous lines except for short distances, that they are staggered on opposite sides of individual discs or rows of granules (e.g. at j) and that they extend parallel to the axis of the chromosome, not in spirals.

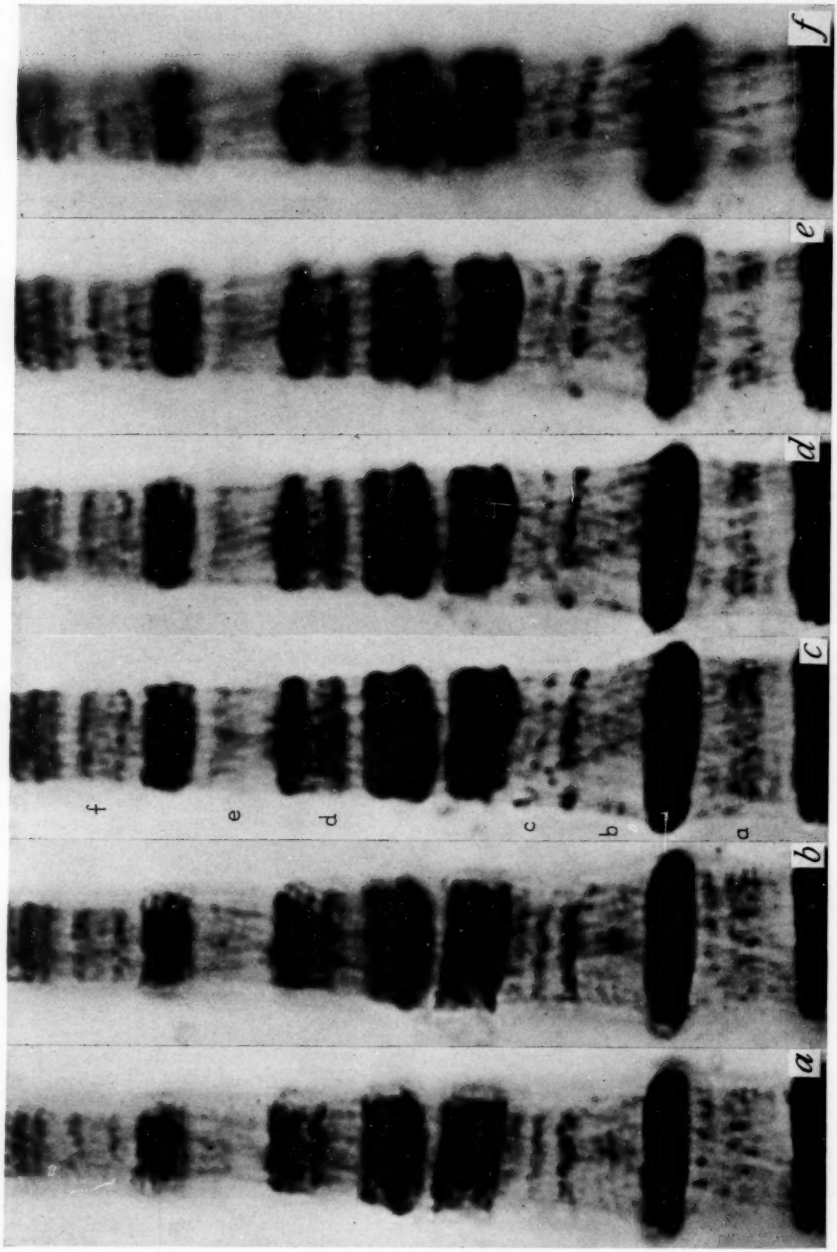



FIGURE 22.

11. In cases of the type just described (item 10) it seems clear that each row of granules is made up of material from two different discs and that each disc contributes material to two different rows of granules, one on each side of it. This seems especially significant, particularly in view of the fact that both types of condition may apparently be found in the same region of a chromosome in different cells—i.e. the material which forms a granular disc in one case may apparently in other cases go into the formation of two separate rows of granules which lie on either side of the space which would otherwise be occupied by the disc (Metz 1935b). The region shown at *d* in Fig. 13 shows such characteristics. It will be considered in detail elsewhere. (See also legend, Fig. 21.)

12. Often small heavy-walled droplets resemble granules, and may even be indistinguishable from true granules on superficial observation. Such structures, however, are in all essentials like the droplets considered above and form a part of the alveolar or honeycomb structure. A disc made up of such "granules" is, of course, double.

13. When a giant chromosome is distorted by stretching, twisting or shrinkage the honeycomb structure described above becomes correspondingly distorted (Metz 1936) and striations of various types may become evident. As in the case of striations in other types of protoplasm, the granules lie in the striations (Figs. 19, 20, 22). These striations have been described as chromonemata by several authors, as intimated above. Our observations indicate, however, that they are not chromonemata and are not really thread-like in nature. Fundamentally they represent the interfaces, with more or less chromatic material, between the droplets separating one disc from the next, as indicated in the figures just cited. With progressive stretching such a structure becomes more and more disrupted, first giving a criss-cross or basket work appearance, then a series of striations which become more and more parallel as stretching or shrinkage increases.

Conditions have been described by Bauer (1935, 1936) in *Chironomus* which are said to give clear indication of the presence of chromonemata. Through the kindness of Dr. Bauer we have been permitted to examine the preparations in question. They appear clearly to illustrate the type of organization described above—a criss-cross of interlacing lines—and the same type of fibrillar structure is visible in parts of the cytoplasm, including the gelatinous secretion product of the glands. It is felt that in both the chromosomes and the non-chromosomal materials such apparent structure is the result of shrinkage, together in some cases with mechanical distortion. In this, as in other cases, examples of parallel striations can be found in restricted individual regions, apparently due to localized stresses. As noted elsewhere, however, such striations may extend in any direction, depending on the direction of stress.

FIGURE 22

FIG. 22. PHOTOGRAPHS OF SIX DIFFERENT OPTICAL LEVELS, IN SEQUENCE, OF A STRETCHED REGION IN A SALIVARY GLAND CHROMOSOME IN CHIRONOMUS (SP.), FROM AN ACETO-CARMINE PREPARATION. ×3000

Two additional optical levels showing the same type of structure were photographed, but are not included here. Note that the striations, which superficially give the appearance of threads, are interconnected to form a lacework or criss-cross pattern which seems clearly to represent distortion and disruption of the fundamental honeycomb type of structure seen in undistorted regions and still visible to some extent in parts of the regions designated a and f here. The fact that this criss-cross pattern is present at all optical levels shows that it is not due to overlapping of spirally disposed threads.

We have made a careful study of *Chironomus* salivary gland chromosomes in this laboratory and have found the type of organization comparable in all essentials with that in *Sciara* and *Drosophila*. Photographs of a portion of a stretched chromosome in *Chironomus* are represented in Fig. 22. The criss-cross type of structure here is present at all levels, as seen by focussing up and down through the chromosome, and hence cannot be considered to represent overlapping threads, or to be due to twisting a system of parallel threads. Likewise the striations are not independent, but interconnected, and in favorable regions in such cases the transition from the true honeycomb organization to the more definitely striated is clearly visible, as indicated to some

extent in Fig. 22 in the regions marked a and f.

A more extreme case of stretching is shown in Fig. 20, from *Sciara ocellaris*. Here the original organization has been almost completely disrupted, and the material drawn out so that it exhibits more or less parallel striations. Careful examination shows, however, that these are not continuous, but either interconnect or are staggered. In this chromosome the more delicate discs and granules have become obliterated by the distortion and most of the chromatic material has become incorporated into striations connecting the larger "granules," particularly in the region between h and m. In this region conspicuous individual lines may be traced for short distances, but then abrupt shifts occur. For example, the lines from i to j do not connect with those from j to k, but alternate with them in position. Similarly, those from j to k shift alignment at k, where they end in heavy "granules." The granules of the next row, at l, alternate with those of the latter row in position.

It should be recalled that the material in the chromosome here was coagulated by fixation (aceto-carmine method) before the stretching and distortion occurred. Hence the coarser structures, such as heavy droplets or granules, have tended to retain their relative positions while the finer ones have been shifted or disrupted. In the lower part of the figure, between a and h, the honeycomb structure has been less completely disrupted and more of the interconnections are visible. It will also be noted that the number of striations is considerably larger here than in the region just considered.

It seems clear that in extreme cases like this the number of striations in a region is determined by the number of *large* granules in the transverse rows. In other words, here, as in other forms of alveolar protoplasm, distortion produces striations resembling strands between the larger formed bodies or granules. In the region from b to h in the present case there are no rows of very large granules. All the granular "bands" or discs contain a relatively large number of small granules. Hence the number of striations is large. In the region from i to l, however, there are three transverse rows of large granules, at i, k and l respectively, and the number of striations corresponds to these. It is safe to say that in the original chromosome there were numerous rows of small granules in larger numbers in the intervening region, i to k, and that these have been obliterated in stretching. The one visible row, at j, has more granules than either that at i or at k.

The type of picture obtained by stretching depends to a considerable extent on the original condition of the chromosome. If the chromosome is "vesiculated," as in Fig. 18, and the chromatin largely distributed around the achromatic droplets, stretching produces the type of structure shown in Fig. 19. Here there are few conspicuous granules and the chromatin forms heavy striations which obviously are not threads, but optical sections of thin sheets or strips like the walls of minute, stretched bladders.

The evidence here agrees with that presented previously (Metz, 1936) in indicating that the chromatic material has a high degree of tenacity, may be greatly stretched, and in severely stretched chromosomes is nearly all drawn into striations. This is true in *Chironomus* as well as *Sciara*. In both cases striations may be produced in any direction within the chromosome by suitable stresses, and the chromatin of the discs may be pulled out laterally into delicate strands. An

example of the latter type of distortion in *Chironomus* is shown in Fig. 23. It is significant that here, as in *Sciara*, pulling the material out sidewise does not reveal any indication of longitudinal threads.

It is felt that the important consideration in the present connection is the mode of formation of the striations rather than their appearance in stretched chromosomes. Where the component structures in apparently spiral or diagonal paths, as described by Koltzoff and by Bridges. Such an appearance is found when the structure is of the type shown in Figs. 7 and 8. Moderate twisting of the chromosome accentuates the spiral appearance. It seems clear that such alignment of the granules is of secondary importance and does not indicate the presence of chromonemata.

FIG. 23. PHOTOGRAPH OF A PORTION OF A CHROMOSOME GROUP FROM A SALIVARY GLAND NUCLEUS OF CHIRONOMUS (SP.), FROM AN ACETO-CARMINE PREPARATION. ×1710

This illustrates the fact that lateral as well as longitudinal strands may readily be produced by suitable distortion, and that such lateral distortion does not reveal any indication of longitudinal threads (see text). Lateral strands extend from the region marked a, and also from the chromosome at the upper right.

are small, as in *Drosophila* and to a considerable extent in *Chironomus*, the finer organization is often obscure. In *Sciara*, however, details of structure are often so clear as to indicate clearly the nature of this organization (in the coagulated chromosome).

In some cases the granules in a small region of a chromosome may be aligned

## DIFFERENTIATION OF THE GIANT CHROMOSOMES

Up to this point no special consideration has been given to the possibility that the giant chromosomes are not only enormously enlarged, but are highly specialized structures in which the component parts have undergone radical modification comparable to that exhibited by the cytoplasmic constituents in the same cells. Alverdes (*loc. cit.*) in his early work on the subject considered them to be products of a

process leading directly to disintegration, and interpreted the internal structures on this basis. Although his particular interpretation is not now acceptable his point of view may be correct, for the salivary glands under consideration are larval organs which undergo histolysis at pupation.

The opposite point of view has been maintained by Bauer (1935) who has claimed, on the basis of Holt's early evidence, that giant chromosomes of the type considered here give rise to the multiple chromosome complexes found in the [illegible]ntary canal of *Culex* and that this provides a [illegible]ation of their compound (and hence essentially "normal") nature. Bauer's interpretation has been shown to be erroneous, however, by Mr. C. A. Berger (*loc. cit.*) who has made a careful study of conditions in *Culex* in this laboratory and has found, as intimated above, that the multiple complexes do not arise from structures comparable to the giant chromosomes, and also that cells which do possess giant chromosomes here all degenerate at metamorphosis. These findings agree with all other observations on the subject, so far as we are aware. The evidence is strong, therefore, that giant chromosomes of this type are found only in cells which are incapable of further division. It would not be surprising if this specialization were reflected in a marked modification of internal structure.

It should also be noted in this connection that in *Sciara*, as shown by Doyle and Metz, the giant chromosomes in a normal living condition occupy most of the space in the nucleus and appear almost structureless, while on fixation or aging in physiological salt solution they undergo great shrinkage through loss of non-staining material and at the same time reveal the internal structural characteristics already considered. Observations made here in coöperation with Dr. D. F. Poulson indicate that comparable changes occur in *Chironomus* during fixation or aging. Such considerations, of course, do not imply that structural differentiation is lacking in the living chromosome and is produced by fixation; but they do raise a question as to how the fixation image should be interpreted, and also as to how far conditions in the giant chromosomes are comparable with those in "ordinary" chromosomes. Since much achromatic material is extruded by the giant chromosomes on fixation, it seems possible that much of what remains is not really chromosomal material but nucleoplasm.

At present there is no obvious way of making proper allowance for these features. They serve mainly to emphasize the need of caution in interpreting the evidence or in formulating any comprehensive hypothesis. What appears to be a significant line of evidence in this connection is provided by the chromosomes in some of the relatively small cells of the salivary gland. Like the cells, these chromosomes are many times their original size, although small as compared with the largest ones. They almost certainly represent pairs of homologues, and probably are at least quadripartite (evidence of Painter and Griffen, *loc. cit.*, and unpublished evidence of Dr. J. B. Buck) yet they appear single—each as a row of achromatic droplets held together by chromatic regions of about the same diameter as the droplets. So far as we have been able to observe there is no evidence of the four or more individual chromonemata presumably present in them.

## DISCUSSION

In the considerations outlined above attention has been given to three different interpretations as to the organization of the giant chromosomes of dipteran larvae. A fourth interpretation, the oldest of all, should also be noted. It was advanced by Alverdes (1912) who considered the chromatic discs in the giant chromosomes to be disintegration products formed by breaking up of an original coiled chromonema. Each disc was thought to be one coil or part of one coil of the chromonema. The evidence presented in the papers noted above seem definitely to rule out this view, although it is apparently still supported by some investigators (Heitz, 1934; Sinoto and Yuasa, 1935).

The considerations of the present paper lead directly to the view that the visible striations in the giant chromosomes referred to by various authors as individual chromonemata are not chromonemata.

It seems clear that the striations in distorted chromosomes do not represent any definite units of organization, but are distortion products comparable to those produced under comparable conditions in other protoplasmic materials. The achromatic droplets are units of structure in a descriptive sense, at least, and form a natural basis for attempting to interpret the organization in terms of

chromonemata. The simplest method of doing this would be to assume that the droplets are aligned in a compact series to form chromonemata. As already noted one serious objection to this assumption is seen in the fact that the number of droplets is not uniform at the different loci in a chromosome. Another difficulty is the enormous size of the droplets as compared with any units in an ordinary chromonema. A third difficulty, according to our evidence, is the lack of any visible alignment or attachment or differentiation into such series. A fourth difficulty is seen in the lack of certainty as to whether the droplets have real structural significance and whether the actual numbers or constancy of numbers of droplets fulfills the requirements of such a view. It is by no means certain that the numbers are regularly multiples of two, or that a given locus will regularly have the same number under comparable conditions of size, etc.

Since the present paper was originally written Bauer (1936) and Painter and Griffen (1936) have called attention to conspicuous heavy-walled droplets and considered them as chromomeres aligned on threads. Painter and Griffen state that there are "two types of achromatic areas in the giant chromosomes; one is made up of chromomeric threads connecting the chromomeres (bands), the other formed by the achromatic vesicles." These areas are apparently considered to alternate. Although such an interpretation is suggested by the honeycomb type of structure in some regions our evidence does not support it as a conception of the real organization. Apparently they consider the delicate lines across one achromatic zone as threads and those across the next achromatic zone as interfaces or boundaries between appressed droplets. Our evidence does not indicate such a distinction (see, e.g. Figs. 5, 6, 13, 14, 15).

It might be suggested that the achromatic droplets do have significance as individual structures in the following manner: (1) that in some sense, as yet not understood, they represent chromomeres which have arisen by division of preexisting chromomeres which had themselves undergone great enlargement. (2) That the original chromomeres in one chromonema were intimately united to form a delicate cylinder of essentially uniform diameter—not separate and connected by threads like a string of beads. (3) That in the development of the giant chromosome no new true chromonemata are formed, but the chromomeres at different loci undergo multiplication more or less independently. (4) That growth continues fairly uniformly in all, so that if division does not occur the individual droplets are correspondingly enlarged (thus accounting for large droplets in small numbers in some regions).

Such an interpretation is far from satisfactory, and is opposed by certain lines of evidence (considered elsewhere). It is particularly unsatisfactory in that it necessitates the assumption of relatively enormous growth of the original chromomeres (genes?)—a growth without division, which has no counterpart in ordinary cells. As noted above, in the case of *Culex*, true, uniform multiplication of chromonemata does not give a structure resembling a giant gland chromosome.

As an alternative interpretation it might be suggested that two types of multiplication have occurred, in the following manner: It might be considered that the droplets multiply, as just intimated, but that each is itself a highly compound structure derived by multiplication of finer units which are themselves invisible. As already pointed out, however, such a view not only lacks observational support, but introduces a serious difficulty in that it assumes the presence of units of two

types, one a compound of the other, but nevertheless capable of division. Furthermore, since on this view the real units are invisible, the interpretation reveals little about the real nature of the structure.

Since much emphasis has been laid on the chromatic granules, by nearly all observers, a few words of further comment on them may be added here. The true granules (as distinguished from the small heavy-walled droplets which are only granules in a descriptive sense) seem best interpreted as merely relatively thick, as compared with relatively thin, regions in the discs, or as accumulations of material between the achromatic droplets separating the discs. In both cases they appear to owe their characteristics to the adjacent achromatic droplets, except as regards bulk, which depends also on the total amount of chromatic material present in a given region or disc. It is clear that the granules do not represent units of like characteristics in ordinary chromosomes. They could only be considered to have significance in relation to units in ordinary chromosomes by assuming a great enlargement of these original units. On any other basis they could only be given significance as real structural units of chromonemata by assuming that they represent definite multiples of the original units—i.e. are units of a higher order, but are themselves compound. As pointed out previously (Metz 1937) such an assumption not only seems to have no foundation in the evidence, but is difficult to justify on a theoretical basis.

On the present interpretation the giant chromosome may or may not owe its size mainly to multiplication of chromonemata which retain the size characteristics they exhibit in other cells. If it does, however, the evidence indicates that the unit chromonemata and chromomeres are not visible as such and must be present in very large numbers. Even in this case it seems necessary to assume that the chromonemata have increased in length, as intimated above, for, although theoretically possible, it seems very improbable that a chromonema in an ordinary cell in one of these flies could approximate the length of a giant chromosome.

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# THE SURVIVAL OF TISSUES AFTER THE DEATH OF AN ANIMAL

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ONCE upon a time a person, evidently not a biologist, wrote a jingle to the effect that when his dog Rover died, "he died all over." The rhyme may have been correct but the facts were wrong, because certain it is that when Rover's heart stopped beating he didn't die all over; some of his tissues promptly ceased functioning but others did not.

This point is of interest to the human race for a number of reasons. Thus, when a man has hanged himself, or has been submerged in water, or exposed to carbon monoxide gas, or has apparently been killed by an electric shock or an overdose of chloroform, the question arises: How soon must the heart be put in motion again if resuscitation is to be effected and ultimate recovery is to be complete? Unfortunately, there have been many cases in which, after artificial respiration had been carried on for a time, the heart and lungs resumed their work but the blood returned too late to the brain, and the patient either remained comatose for several hours and then died or else woke to find later that his brain was badly injured. Much information on this point is to be found in articles by Shillito, Drinker and Shaughnessy, Salinger and Jacobsohn, Bruns, Pike, Guthrie and Stewart, Scheven and Boehm.

Some day, when we learn how to prevent the autolysis of transplanted organs, surgeons will be seeking for the best methods of preserving tissues removed from healthy persons who have met sudden death. Already the Russians are experimenting with red blood cells removed from the dead, such cells being kept on ice until needed for transfusion.

The subject of survival is of interest also to the histologist when he is securing bits of tissue for study; how long can he wait after the death of the organism before the cells will have changed in their appearance. Or the physiologist who is interested in keeping tissues alive and functioning normally outside the body may want to know how long he can wait before starting a tissue culture.

I became interested in survival times because I thought I could probably use differences in death rates of the different tissues of the bowel and perhaps parts of the neurones to analyze the structure and functions of the myenteric plexus. For instance, if it could be shown that synapses are unable to transmit impulses after a certain time, then any function that fails at that time might well be ascribed to conduction through synapses.

It was with this idea in mind that the writer searched through much literature for information as to the ways in which different parts of the nervous system die. As everyone knows, the first organ to suffer when the heart stops is the brain; and some parts of it are more sensitive and vulnerable than others. This difference in vulnerability may perhaps be

explained on the basis of local variations in metabolic rate. As one would expect, the tissues which have the fastest metabolism and therefore the greatest need for oxygen suffer first when the supply of this gas fails. One is reminded of Hippocrates' statement that "Old men endure fasting most easily, then men of middle-age, youths very badly, and worst of all children, especially those of a liveliness greater than ordinary."

Because so many of the studies on the survival of tissues are made incidentally in the course of other researches and are not mentioned in the title of the published report, it is difficult to review the subject completely, and I have not attempted to do so. As I said before, my main purpose in making this study was to gain some idea of the survival times of the different parts of neurones, and particularly of the peculiar types of hardy neurone that is found in the sympathetic nervous system and in the plexuses of the intestine.

Naturally, the rate at which a tissue dies depends not only on its own metabolic rate, but also on the metabolic rate of the animal of which it is a part. Thus it has been shown that the tissues of the quickly moving fishes die faster than do those of the more sluggish ones (Willer), and, as one would expect, the tissues of a cold-blooded animal usually survive much longer than do those of a warm-blooded animal. Jolly found that the blood cells of Triton kept forming new mitotic figures for fifteen days after removal from the animal, and the leukocytes of batrachians still showed ameboid movements after ten months in the ice box.

Differences in the metabolic rates of the several parts of the bodies of lower forms of life have been studied extensively by Child and his pupils. MacArthur and Jones showed that in the central nervous system the metabolic rate of the tissues decreases as one passes from the gray matter of the cerebrum to the white tracts at the lower end of the spinal cord, and a corresponding gradient in the vascularity of these parts was shown by Craigie.

As one might have expected, a number of studies showed that the speeding up of the metabolic rate of tissues by the giving of thyroxin impaired their ability to withstand anoxemia, and the slowing down of the metabolic rate effected by thyroidectomy lengthened the survival time in a poorly oxygenated medium (5, 20, 41, 98).

The survival time of a tissue depends also on the temperature at which the tissue is kept after death. Grodzínski, who kept bits of aorta taken from chick embryos at a temperature of 38°C. could not get them to grow in tissue cultures started after three hours. Bucciante found that moderate cooling down to 0°C., would lengthen the period of survival, but great cold, below 0°C., greatly shortened this period. When they had been cooled to −30°C., none of the bits of tissue removed from chick embryos grew. The survival time depends also on the amount of care that is taken to avoid contamination of the tissue with bacteria. Furthermore, men who kept tissues in oxygenated Locke's solution reported longer survival times than did those who first boiled and sealed the fluid, or who replaced its oxygen with nitrogen (Garry, p. 235). Nolf found that the highly sensitive ganglion cells in the intestine of the chicken survived better if some blood was added to the iced Locke's solution, and Cannon and Burket found that when they squeezed all the blood out of a short segment of bowel the survival time of the ganglion cells was about half of what it was when the mesenteric vessels were simply tied. The writer found that when the lumen of excised intestine was kept full of oxygen,

the functions of irritability, conduction of impulses, and rhythmicity were retained for many hours. Oxygen under increased pressure has been used by some investigators to keep excised tissues alive.

Because of these variables, one cannot always compare the figures given by one investigator with those of another. Thus, the men who, in studies on animals, tied both carotid and both vertebral arteries observed a longer survival of the reflexes about the snout than did those who used a guillotine. This was because even the small amount of blood that trickled into the brain through the spinal arteries served to prolong the life of the ganglion cells (Hill, Gildea and Cobb and others). Winkelbauer, who bled dogs until the electrocardiogram showed that the heart was no longer pumping, found that few animals would recover if the blood was not returned by rapid transfusion within five or six minutes. Curiously, an occasional animal could be revived after twenty minutes. On the other hand, Bruns was able to resuscitate dogs which a half hour previously had apparently been killed by the inhalation of carbon monoxide or carbon dioxide. In them, some circulation must have persisted for a while.

The length of time which may be allowed to elapse between apparent death and successful resuscitation will naturally be influenced by the length of the interval during which the heart keeps beating strongly enough to maintain a small flow of blood in the brain. Thus, in the cases of six dying men, Laubry and Degos found that some electrocardiographic signs of cardiac activity persisted for six, seven, ten, twelve, fifteen, and twenty minutes, respectively, after respiration had stopped and the radial pulse had disappeared (78). When men are officially hanged by being dropped through a trap, the heart continues to beat for about twelve minutes (94).

Remarkable experiments have been performed by a number of men who have markedly dehydrated certain tissues, or even entire small animals, and have later moistened these tissues and caused them to live again. This ability of lower forms of life to dry out and live through a period of drought was commented on years ago by Leeuwenhoek. According to Hall, some investigators have resuscitated small forms of life kept in a dry state for as long as five or six years. Even more remarkable is the report of Baker who, in 1764, moistened some nematodes that had been kept dry for twenty-seven years and found that they could be brought to life again (Hall).

Hall did a great deal of work to see how much dehydration would be tolerated especially by various lowly animals. According to him, man will tolerate the loss of only 10 per cent of his tissue fluids. More recently Krawkow, Morosow, and others have reported almost unbelievable survival times of bits of chick embryo, heart muscle and blood vessel wall kept in a desiccated condition for periods as long as six months. Morosow proved that his desiccated tissues were alive by culturing the cells. According to Lipschütz, partially dried ovaries kept for months and then transplanted into a suitable animal grew and produced recognizable hormonal effects.

### THE BRAIN CELLS

The cells of the body least resistant to anoxemia seem to be those of the higher centers of the brain. They are so dependent on a good supply of blood that, in both man and animals, consciousness often fails the moment the circulation stops. The patient who suffers a large coronary thrombosis, or a temporary stop-

page of the heart beat such as is seen in coronary thrombosis or Adams-Stokes' disease, may topple over in the middle of a sentence, and the aviator, whizzing around a pylon in the international speed races, or straightening out after a long dive, "blacks out" or goes blind for a second as the centrifugal force pulls the blood out of his brain. Willius tells me that in the case of a man with Adams-Stokes' disease, reported by him and Yater, the stoppage of the heart beat was followed promptly by unconsciousness; the patient would stop in the middle of a sentence, only to go on a minute later as if nothing had happened to interrupt his flow of thought. In one such episode the electrocardiogram showed that there had been no heart beat for almost four minutes. Gallavardin and Bérard reported a similar case in which a man survived periods of asystole as long as two and a half minutes. The experiences of Nathanson, Weiss and Baker, Smith and Moersch and others who stopped the heart beat by pressure on the carotid body showed that some persons can remain conscious for from seven to twenty seconds after the circulatory pump ceases to function.

With lesser degrees of anoxemia, such as are met with at high altitudes or in cases of carbon monoxide poisoning, the higher faculties fail first and the victim does not recognize the fact that he is in danger. A beautiful example of this is to be found in Stefansson's vivid story of his difficulties in getting his fellow explorers out of an ice hut in which they were all being badly gassed by a defective stove.

In 1857 Brown-Séquard found that when rabbits were made unconscious by compressing for three minutes all four arterial trunks supplying the brain they could rarely be resuscitated. More recently, Hill found that the degree of cerebral anemia produced in a wild rabbit by holding it up by its ears was enough to paralyze the respiratory center in from eighty-five to 110 seconds. Complete anemia of the brain, if maintained for one minute, abolished all cortical activity.

After stoppage of the circulation the pupillary and ocular reflexes disappear almost immediately, but the respiratory and vasoconstrictor centers continue to show some signs of function for from thirty to 120 seconds (7, 9, 30, 39, 96). Pike, Guthrie and Stewart, Stewart and Pike, and others have found that the respiratory center is unusually resistant to anoxemia.

Laborde, experimenting with the head of a decapitated criminal, found that electrical stimulation of the cortex of the brain caused contractions of the facial muscles for a period of fifty minutes. In another such head the cortex lost excitability in twenty-five minutes, leaving only the facial nerves capable of function. Much of the old literature on this phase of the subject is given by Battelli, d'Halluin, Prus, and Hayem and Barrier.

The next question is: How long does it take for anoxemia to injure the brain cells so severely that they cannot recover? The best answers seem to range from five to ten minutes (30, 31, 39, 45, 93, 96). Reports of longer intervals indicate that the experimenter failed to shut off the circulation completely. Even when the anoxemia lasts only from five to ten minutes many of the animals that survive are demented.

### NERVE CELLS IN THE SPINAL CORD

The nerve cells in the spinal cord are apparently a little more resistant to anoxemia than are those in the brain, but still they are so sensitive that closure of the aorta causes immediate insensitive-

ness and paralysis of the hinder parts of the body, together with a loss of reflexes (31, 93). Recovery is not likely to be complete if the obstruction is maintained for more than ten minutes. Some return of function can take place after thirty minutes, but after an hour the nerve cells are destroyed.

According to Ehrlich and Brieger, in rabbits one can destroy electively the gray substance of the cord if one shuts off the circulation for the right length of time. The conducting paths and the ganglions on the posterior roots are considerably more resistant than are the anterior horns.

### SYNAPSES IN THE CENTRAL NERVOUS SYSTEM

Now that the synapse is coming to look more like a drugstore and less like two interdigitating sets of roots (6, 29, 84), it is not so easy to talk about its injury and "death"; all one can say is that it is less resistant to anoxemia and other deleterious influences than is the rest of the neurone (90).

Recently Heinbecker found that synapses can be blocked by a dose of carbon dioxide which does not extinguish the action potentials of normal nerve fibers, and he used this fact to help him in analyzing the structure of certain parts of the nervous system. The fact that reflexes disappear usually within a few seconds after the stoppage of the circulation indicates that synapses in the central nervous system are highly sensitive to anoxemia.

### NERVE FIBERS AND ENDINGS IN THE CENTRAL NERVOUS SYSTEM

According to Forbes and Ray, if an animal is killed and the nerves are left in situ, action currents cannot be elicited after the passage of one or two hours. Curiously, if the nerve is removed after it has lost all signs of function and is placed in Locke's solution, it will recover and will then survive longer than if it had been removed promptly after the death of the animal. Nerves promptly removed and kept in Ringer's solution at a temperature of from 5 to 10°C. showed action currents as late as the fourth day. Symes, using cats and dogs, saw negative variations in excised sciatic nerves kept at 0°C. for from seven to nine days and then warmed. Gerard kept dog's nerves in an atmosphere of nitrogen and at a temperature of 25°C., and he found that the strength of the electric response was reduced by half in from ten to forty minutes. The survival time varied with the metabolic rate of the nerve (26, 46).

According to Hermann's old studies, the voluntary muscles can be stimulated through the nerves for about an hour after death. After tying the aorta of a rabbit, Fredericq stimulated the sciatic nerve and could obtain muscular responses from the muscles after half an hour. De Zilwa obtained contractions of the retractor penis of a dog on stimulating the pudic nerve two hours after death. Curiously, Parker found that the neurofibrils in nerves go to pieces a few seconds after the stoppage of the circulation.

In man, the sudden closure of the femoral artery by an embolus produces immediate anesthesia of the affected limb, anesthesia which appears to be due to an injury to the sensory nerve endings and not to the nerve trunks. The motor functions of the limb are not much disturbed, which again indicates the comparative hardiness of nerve fibers and motor nerve endings. Curiously, the limb which is anesthetic to a pin-prick will usually for some time be the seat of agonizing pain, showing that sensory nerve fibers are still conducting (88).

### RESISTANCE TO ANOXEMIA IN PARTS OF THE AUTONOMIC NERVOUS SYSTEM

Available evidence points to a greater hardiness of autonomic neurones as compared with those in the central nervous system, and to a greater hardiness of the sympathetic neurones as compared with those in the parasympathetic division (42, 43, 55, 70, 89). Those in the wall of the intestine seem to be hardiest of all.

Elliott found that four hours of anoxemia due to clamping of the aorta did not seem to injure the function of the ganglion cells on the bladder of the cat. Years ago, Langendorff, and later Hering and Langley, showed that when a cat or rabbit is killed, the ocular reflexes that are dependent upon the functional integrity of synapses in the sympathetic system disappear in from ten to twenty minutes, together with the pilomotor effects of stimulating preganglionic fibers.

The normal effects of stimulating postganglionic fibers can be obtained for from thirty to forty-five, or perhaps even ninety minutes, after the death of the animal. In order to restore function in the cervical ganglions, Schröder had to perfuse the neck usually within fifteen or twenty minutes after the stoppage of the circulation. In only one experiment did he get a return of function after an hour.

In one experiment Hering found the cervical ganglions impermeable to stimuli within fifteen minutes after the stoppage of the circulation; the postganglionic fibers were still conducting after thirty-three minutes, and the vagus fibers to the heart were active for fifty-five minutes.

Pavloff cut a vagus nerve and left the end under the skin of the neck where he could stimulate it. The slowing effect on the heart was lost in from four to five days, but the effect on the pancreas persisted until the eighth day. This was confirmed by Tonkich. Using a similar technic, Arloing noted slowing of the heart up to the eighth day in some animals. In the dog all effects were gone by the fifth day. Curiously, in a donkey, long after the effect on the heart was gone, in fact thirty days later, there were good effects on the larynx and esophagus. All of this suggests that certain types of nerve endings in the tissues can degenerate before the fiber does.

According to Hering, Danilewsky and others, if a dead dog or monkey is kept almost frozen, and the heart is later revived by transfusion, a vagus effect can be demonstrated if the interval has not been more than one or two days; the accelerator nerves usually survived longer, in one instance up to fifty-three hours. If the body is not frozen immediately, the vagus effect on the heart seems to be lost in about forty minutes, and the accelerator effect a little later.

From work on guinea-pig bowel, Newman and Thienes came to the conclusion that the vagus nerve endings in Auerbach's plexus probably lose their function rapidly after the circulation stops. The nerve endings in the retractor penis of the dog were effective for at least two hours after the death of the animal, and they were immune to the largest doses of atropine and curare (107).

### THE NERVES OF THE INTESTINE

Mall found, in studies on animals, that if the superior mesenteric artery was clamped for from twelve to twenty-four hours, reëstablishment of the circulation no longer started rhythmic contractions; but the bowel was not dead, as was shown by the fact that when he injected some hot saturated solution of mercuric chloride into the lumen he produced powerful rhythmic and peristaltic contractions. Cannon and Burket showed that segments of the small bowel of a cat or dog will

survive for about six hours even when all the blood supply is tied off. So far as they could see, the functions of such segments were normal after the restoration of the blood supply, and they could not find in microscopic sections any degeneration of the ganglion cells. But such tying of the mesenteric vessels leaves blood and hemoglobin close to the nerves, and since this almost certainly saves them from injury, Cannon and Burket devised other experiments in which they exsanguinated short segments of bowel by compressing them between two glass slides. By doing this they cut the survival time of the ganglion cells to about three and a quarter hours. They concluded that any period of exsanguination short of that which produces gangrene of all the tissues will leave the nerves capable of recovery, and this indicates that the nerve cells of Auerbach's plexus must be the hardiest in the body. Similarly, Scandola found that the intestine of the dog would survive six hours of anoxemia, and three weeks later the mistreated segment behaved normally.

Elliott clamped the aorta of a cat for four hours and failed to kill the ganglion cells that are to be found on the wall of the bladder. Macht found that the pigs' ureter will contract rhythmically after three days in the ice box.

According to Nolf, the neurones in the excised intestine of the chicken are so sensitive to anoxemia that the tissue must be kept very cold and in contact with blood if one wishes to revive the nerves later. Curiously, in the chicken, the nerves of the intestine seemed to be much less tolerant of anoxemia than was the sciatic nerve.

### SURVIVAL OF MUSCLE

Hering, Danilewsky, Kuliabko, and others almost froze recently killed animals, such as dogs, rabbits and monkeys, and made the heart beat again after two or three days. Kuliabko and d'Halluin caused the hearts of infants to beat again twenty hours after death. Luciani mentions the case of a criminal whose heart was made to beat eleven hours after death.

Gunn and Underhill, Alvarez and others resuscitated intestinal muscle from cats, dogs and man, which was kept for about five days in cooled Locke's solution, and caused it to contract rhythmically. Overton and Mines preserved the irritability of excised sartorius muscle of frogs for from sixteen to twenty-one days by keeping it cool and under sterile conditions. Overton used a solution of 0.75 per cent boric acid to inhibit the growth of bacteria. Similarly I have kept rabbits' intestine irritable for a week in Locke's solution to which was added .05 per cent boric acid. A concentration of 0.5 per cent was injurious. The removal of the intestinal mucosa also served to prolong the life of the muscle.

Tennant ligated the descending branch of the left coronary artery of animals and found that the muscle lost its power to contract effectively after one minute. This change was irreversible after twenty-three minutes. Marked changes took place in the excitability, for electric currents, of the dog's sartorius after seventy minutes (8). Factors influencing the survival of intestinal muscle after the death of an animal were studied by Ascanio and me, by Garry, and by Gross and Clark.

### HISTOLOGIC DETERMINATION OF SURVIVAL TIME

Besides the physiologic method of determining survival time there are histologic methods. Thus, Cannon and Burket found that in exsanguinated gut, the ganglion cells showed marked degenerative changes after three and a half hours.

Curiously, however, Müller found that such cells would stain well in intestine removed from ten to fifteen hours after death, and van Esveld obtained well-stained preparations from segments of bowel that had been kept well cooled for seven or eight days. Perhaps in such cases the stagnant blood in the bowel helped to preserve the tissues. In my own work I found it almost impossible to tell from histologic study whether or not ganglion cells are capable of function. When they were edematous and cloudy, and Kernohan, an experienced neuropathologist, was sure that they were decidedly abnormal, I had to admit that the intestine from which the sections had been taken had seemed to function normally.

Lewis and McCoy removed the various organs of rats aseptically, and in one group of experiments kept them in a moist chamber at 37°C. These experimenters used as a sign of dying the development of vacuoles and granules which had an affinity for neutral red. When the tissue died, the color left the granules and vacuoles and diffused throughout the cell. In the brain the granules and vacuoles seemed to form before the experimenters had time to get the tissue out of the dead animal, and a similar difficulty was encountered in the case of intestinal epithelium and heart and skeletal muscle. The principal cells of the liver and the pancreas showed degenerative changes after ninety-six hours, but in smooth muscle from the bladder and uterus they came only after 240 hours.

Gomez and Pike, and Gildea and Cobb found decided histologic changes in the ganglion cells of the brain within ten minutes after death. Cells in the medulla and in the anterior horns of the spinal cord lasted a little longer, and cells in the spinal ganglions lasted thirty minutes. According to Tuckett, sympathetic ganglion cells had to be deprived of their blood supply for five days before they showed histologic changes comparable with those produced in the spinal cord in one or two days.

Another way of determining whether a tissue is alive or dead is to culture the cells and see if they will grow. Thus Bucciante took bits of chick embryos and left them under sterile conditions at room temperature for varying periods of time. He found that skeletal muscle would grow after twenty days, cornea after eighteen days, skin after fourteen days, heart muscle after six days, and liver after two days. These periods of time could be somewhat lengthened by keeping the tissues in the ice box.

In a long series of experiments my associates and I found that anoxemia can be used much as nicotine is used to block synapses, and to throw light on the structure and functions of neuromuscular mechanisms like the heart and bowel. With the body of the dead animal or the excised bowel kept moist at 38°C., what were probable synapses between the vagal neurones and those of Auerbach's plexus failed to conduct from ten to twenty-seven minutes after the stoppage of the circulation. Another mechanism, perhaps a synapse, which is more or less essential in the production of peristaltic rushes, often failed to function after from thirty-five to forty minutes. Nervous paths, which in the intestine conduct an impulse about 15 cm. orad and caudad from a point stimulated electrically, often failed to function after 100 minutes; long distance rapid conduction from one end of the bowel to the other was retained for at least seven hours; nerve endings in the muscle also lasted seven hours or more, and the function of rhythmic contraction often persisted for several hours after the disappearance of conduction.

SUMMARY

The literature reviewed indicates that in mammals, including man, endings in the central part of the nervous system and perhaps some ganglion cells, cease functioning within a minute or two after the circulation stops. The susceptibility to anoxemia decreases somewhat as one passes from cortex through the medulla and to the spinal cord. Consciousness and most reflexes disappear immediately after stoppage of the circulation. After from five to ten minutes the damage done to the nerve cells of the brain is irreversible. The nerve fibers and the motor nerve endings of the central nervous system are much more hardy, and will function for an hour or two when left in the uncooled animal.

In the autonomic part of the nervous system, and particularly in the sympathetic division, all parts of the neurone are more resistant to anoxemia than are the corresponding parts of the neurones in the brain and spinal cord. Reflexes persist for from ten to twenty minutes after the stoppage of the circulation, and ganglion cells can be resuscitated for periods of time usually up to twenty minutes. Stimulation of the vagus nerves continues to slow the heart for about forty minutes, while stimulation of sympathetic nerves accelerates the beat for a few minutes longer.

What is probably a synapse at the end of the vagal fibers fails to function from ten to twenty-seven minutes after the stoppage of the circulation. What is perhaps a synapse, connecting neurones in Auerbach's plexus and helping in the production of peristaltic rushes, commonly fails to function after from thirty-five to forty minutes. Nervous conduction in the bowel lasts often for more than seven hours. Even then, if the blood is left in the vessels the changes in the nerve cells are not irreversible and several days after removal of the clamp on the mesenteric arteries all the motor functions of the bowel will seem to be normal. If all blood is removed from the vessels, irreversible changes appear after three hours and a half.

Because of their slower metabolic rate, the tissues of lower forms of life stand anoxemia and dehydration much better than do the tissues of higher forms.

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# A CRITIQUE OF PLANT SEROLOGY (*Continued*)

## PART II. APPLICATION OF SEROLOGY TO THE CLASSIFICATION OF PLANTS AND THE IDENTIFICATION OF PLANT PRODUCTS

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[*Editorial Note:* Exigencies of space make it necessary to publish this review in three parts. Part III will appear in the next number following this. The extensive bibliography will follow Part III.]

### I. IDENTIFICATION OF PLANT PRODUCTS

*SUITABILITY of serology for purposes of identification.* The high degree of specificity of the serological reactions as familiar to students of the serology of bacteria and animal proteins, extends to the higher plants as well, although the relative complexity of plant extracts is such as frequently to obscure the specificity to a considerable degree. Thus one commonly finds in the phytoserological literature statements to the effect that animal proteins are more highly specific than plant proteins. We know from the works of Wells and Osborne that provided plant proteins are obtained in a state of comparative purity they exhibit a degree of specificity entirely comparable with that of egg albumin or the proteins of blood. Indeed familiar examples of the species-restricted character of hay-fever sensitivity afford excellent illustrations of the narrowness of the limits of serological affinities even among closely related species of plants.

The plant extracts commonly used in phytoserological practice, however, represent aggregations of many separate antigens, each presumably capable of evoking the production of antibodies. The serum prepared from such an antigenic extract is proportionately complex in its antibody constitution. We know further that in the animal kingdom the same protein may occur, particularly in a nutritional or other non-specific character, in more or less distantly related species (eye-lens protein, milk proteins), and so in plants one may perhaps find such non-specific components of the protein complex (369). We may think of the plant extract as comprising certain antigens so intimately connected with the individuality of the species that their species-specificity is complete, together with other antigens of a less individualistic character, shared in common with related species. As the degree of relationship between two species decreases, the number and quantity of common proteins decreases proportionately until serological inter-reactivity is no longer observed.

For the purposes of convenience, the present discussion arbitrarily falls into two divisions, concerned respectively with phytoserology based on identity reactions and phytoserology based on relationship reactions. However, it is probable that actually all true serological reactions are identity reactions, and that the observed

serological relationships of plants are due to comparisons of mosaics of antigens in which certain constituents are common to several species, others peculiar to a single species. For a more detailed development of this concept the reader is referred to the works of Moritz (233, 235).

The serological reactions have been found generally suitable for purposes of the identification of plant proteins and plant species. This is dependent on the fact that the proteins peculiar to the species are of sufficient antigenic importance in tissue extracts as to minimize the effect due to non-specific proteins. The utilization of such identity reactions for practical purposes has been attempted with success by a number of investigators, and the findings are considered below.

*Detection of constituents and contaminants of flours, meals, fodders, drugs, etc.* The possibility of identification of proteins by the precipitin and other serological tests has led to many attempts in the utilization of such tests for the determination of the presence and nature of biological materials. As regards substances of animal origin, it has been found not only possible but practical to utilize serological methods for the determination of the source and adulteration of blood, meats, bones, sausages, meat pastes, meat extracts, milk and milk products, eggs and egg products (e.g., caviar, roe, noodles, mayonnaise), honey, animal fats and oils (through their content of proteins as contaminants), and numerous other such substances (194, 260, 353, 216, 354). The routine use of such tests has become somewhat more familiar in Germany than in the United States, and serological evidence has frequently been introduced and accepted as conclusive in German legal cases dealing with pure food and drug laws.

Numerous similar studies have been made regarding plant products, and involve chiefly questions concerning the constituents of flours, meals, bran, animal fodders, and the like. The various plant fats and oils may also be studied from this viewpoint because of their customary content of specifically reacting protein contaminants (194).

As early as 1902 (25, 26, 27) Bertarelli had shown that with legume meals the nature of the gross constituents and the presence of practically important leguminous adulterants can be determined more accurately by means of the precipitin reaction than by the aid of the microscope. Using this method he was able to detect, for example, 2 per cent of *Vicia sativa* in wheat flour. At about the same time, Ottolenghi (267, 268) similarly succeeded in the detection of ergot as a contaminant of flour, although Schern's findings regarding detection of ergot by anaphylaxis tests were less satisfactory apparently because of the very small amount used (314, 315). The precipitin test has also proven satisfactory for the determination of the adulteration of wheat flour by *Vicia faba* (187), maize, rye, barley (189, 347), potato flour (313, 312), and the poisonous *Agrostemma Githago* (23). The toxic castor oil bean, *Ricinus communis*, is a not infrequent contaminant of certain types of European fodders, and the presence of the toxin can be satisfactorily determined by means of the precipitin (Krãnich and Mooser in 312, 226, 243) and anaphylaxis (314) tests, and possibly also by the fact that ricin agglutinates normal red blood corpuscles, while the usual constituents of fodders do not (227). If the toxin is heated to 100° (moist heat) it is no longer toxic and fails to give the precipitin reaction, hence the test is indicative of the edibility of fodders (with respect to ricin) whether they once contained active ricin or not (226, 315).

The amounts of contaminants which can be detected serologically are often so small as to be negligible practically.

Thus Becker could safely detect .36 per cent of *Agrostemma Githago* in flour, .74 per cent in wheat bran, and .15 per cent in spelt, quantities which were no longer distinguishable chemically. Ricin can be safely detected in amounts too small to intoxicate cattle (315, 314), .1 per cent by the precipitin and complement fixation tests (229, 28, Krãnich in 312). 4 per cent, a legally-permissible amount of potato flour adulteration can be detected in wheat flours and even cooked flour products if denatured potato protein is used in

immunisation (312). The gross-allergy test has also proved of service in the same connection, and the field mustard, another common contaminant of fodder, may be recognized by this method (315, 314), although results were less satisfactory with *Agrostemma* (314). Thöni and Thaysen have reported indifferent results in attempting to separate *Plantago* from wheat flour by the use of whole extracts (346), but on using antigens prepared by fractionation of the extracts with ammonium sulfate, they showed the possibility of detecting 3 per cent of such adulteration, which is an amount smaller by 3 times than the least amount which can be identified microscopically (347). Cao in 1904 (42) attempted to differentiate starches to the same end by the effect on the reduction of Fehling's solution of the addition of "starch-immune" serum to starch extracts. The results, however, are by no means convincing.

On the whole it may be said that the precipitin and other serological techniques have been shown to be of real value in the study of the composition of various plant products, the sensitivity and the specificity of the reactions exceeding those of other chemical and physical tests applied to the same materials, and hence justifying their use in a practical way as an aid in the solution of such problems as have been recounted above.

*Application to seed testing.* In 1911 Relander (291, 384) used the precipitin technique in an attempt to differentiate and identify the seeds of 27 varieties of barley, oats, *Vicia*, clover, and lupin. In all cases he succeeded in clearly separating the various species involved, and in some cases the varieties of a single species, but the relationship reactions between different varieties of the same species were often so strong as to prevent the possibility of their differentiation. Similarly, Schmidt in 1926 (316) tried to differentiate the seeds of *Pinus silvestris* of a number of varieties and proveniences, but here again, while the results gave some indication of a separation, the degree of differentiation was often so slight as to render the method insufficiently accurate for this purpose.

It has been the general experience of plant serologists that varieties of a single species are separated serologically only with great difficulty. Since in the problem of seed testing the matters of variety and provenience are of paramount importance while species differences are usually recognizable by gross morphology, it is doubtful whether serology can afford much aid to the other techniques available for the determination of the nature and origin of seed.

*Identification of the plant viruses.* Recent studies are showing that serology bids fair to become a useful technique in the identification of the plant viruses. Beale (283) has already shown that both ordinary and attenuate tobacco mosaic virus may be detected by this means, and Matsumoto and Somazawa (204, 205, 201) have reported at length experiments in which the invasion of plant tissues by this virus was determined serologically before symptoms appeared. In the writer's experience serological tests, particularly the precipitin reaction, have been repeatedly used with success in practical problems in connection with the plant viruses, and the following cases will afford illustrations of the utility of such tests (50, 54):

Masked tobacco mosaic virus in tobacco and mild strains of latent mosaic virus in tobacco, potato, and *Datura* were each identified repeatedly, although in every case the plants bore no recognizable symptoms of disease.

The following viruses in expressed sap were submitted as unknowns (although known to the donor), and each was correctly identified as to group by the precipitin test: tobacco mosaic, tobacco ring spot, cucumber mosaic, latent mosaic of potato, potato ring spot, potato vein banding, and mixtures of tobacco mosaic and potato mottle, of tobacco mosaic and cucumber mosaic, and of tobacco mosaic and potato ring spot. In each case the accuracy of the serological tests was confirmed by infection experiments. Manil (193b) and Matsumoto (201b) have also shown the possibility of identifying the components of virus mixtures in plants by means of precipitin tests. The precipitin reaction has also been used successfully in determining the viability of stored virus extracts and of samples of virus subjected to various physical and

chemical treatments and in studying the transmission of viruses through infected seeds (193c).

Some advantages of the serological tests in such cases are that the testing requires but an hour or less as compared with several days or much more required by the infection method, that one requires no plants for inoculation, which might occasion weeks or months of delay rather than days, that the test is perfectly objective, the symptoms in no way leading to false conclusions, that one may obtain not only a qualitative but to a certain extent a quantitative conclusion regarding the virus present, and that so far as tested virus always reacts with immune serum regardless of whether the virus has been purified by various techniques or preserved in various ways, including drying, so long as the virus is still viable, or whether it has been propagated in hosts different from that used in the preparation of the serum.

### II. APPLICATION TO PLANT SYSTEMATICS

*Early sero-systematic studies.* The first impetus to the use of serology as an aid in plant systematics came as a consequence of the classic studies of Nuttall (1901–1904) demonstrating that by the use of the precipitin test it is possible to show clearly and objectively the relationships of the many hundreds of animal species tested (257–260). The Königsberg series of sero-diagnostic studies commenced with the work of Gohlke in 1913 (104). Between 1901 and 1914, however, there were a number of scattered sero-diagnostic researches, the findings of which are considered in the present section.

In 1901 Kowarski (157) found that the heat-resistant albumose of wheat induces in rabbits the production of precipitins which react strongly with wheat albumose extracts, and weakly or not at all with the albumoses of rye, barley, oat, and pea, although the reaction with pea extract was stronger than that with oat extract. Magnus and Friedenthal next showed by the precipitin test that there is a stronger serological affinity between yeast and the truffle, both of which are Ascomycetes, than between either and a representative Basidiomycete (188). They also confirmed Kowarski in finding that rye serum reacts more strongly with rye extracts than with those of other grains, and further that rye seed serum and rye pollen serum each reacts strongly with extracts of such diverse organs as rye seeds, pollen, roots, and sprouts (190). The same workers also investigated the discrepancy noted above in Kowarski's tests, and found that wheat serum reacted strongly with wheat extract but not with pea extract, and reciprocally. Strong homologous reactions and negative or weak heterologous reactions were also obtained with numerous other plant preparations, e.g. *Ustilago* sap, *Sauromatum* tuber sap, *Mucor* pulp, *Cocos* endosperm milk, and seed extracts of numerous Gramineae (189).

Using Kowarski's techniques, Bertarelli in 1903 (25, 26, 27) performed a number of cross-precipitin reactions with legumes and found that in each case tested the reactions of a serum were much stronger with its homologous extract than with various heterologous leguminous extracts. This work was continued in 1908 (290) by Relander who likewise reported success in the differentiation of species and varieties of barley and *Vicia* by means of the precipitin reaction. Similar results were obtained with species of legumes and grains by Gasis (100) although Gasis observed weak reactions with somewhat distantly related antigens, which according to Magnus (187) were non-specific, normal serum reactions, removable by absorbing the aberrant extracts with normal serum before testing. Such absorption completely restored the specificity of the tests. Magnus also was the first to point out that in the progressive immunisation of an animal, the serum first shows reactivity only with closely related antigens, later with more and more distantly related ones, the homologous reaction always remaining strongest.

Schütze in 1901 and later (321–324) was able to demonstrate both agglutinins and precipitins with yeast preparations as antigens, but was unable to differentiate to satisfaction the various types of yeast used. This may in large part be due to the fact, as pointed out by Defalle in 1902 (64), that the antigens involved are wall substances and not the specific protoplasts or their constituents. Axamit (11) also report positive anaphylactic tests using yeasts as inoculum, although his data on intervals of inoculation indicate that his reactions were atypical if they were truly anaphylactic. Rosenau and Anderson

(297), however, reported reliable positive anaphylaxis tests with yeasts.

In the determination and differentiation of a number of legumes and Gramineae the gross anaphylaxis reaction was used with some success by Rosenau and Anderson (297, 298), Azuma (12), and Karasawa (143), and the precipitin test by Kanahara (142) and Hiki (125).

In 1910, Ballner and Burow (14, 15) applied the complement fixation technique to preparations of legumes and grains, obtaining homologous reactions with titers to 1:20,000, and showing by this method that rye is serologically very close to wheat, with barley and oats less closely related to rye, and rice and maize still more distant although showing some relationship as contrasted with the legumes. The legumes tested were found to form a compact serological group separating into subgroups about *Pisum*, *Phaseolus*, *Vicia*, and *Lens*, respectively. The same year Chapman (46) immunized rabbits to *Acacia* seed extracts, and after discarding all extracts which reacted with normal serum showed that by the precipitin test *Acacia* has stronger affinities with *Pisum* than with *Phaseolus*, *Vicia*, and numerous non-leguminous species.

Wendelstadt and Fellmer (370) applied both the precipitin and complement fixation tests to grains and legumes, and the results obtained confirmed those of the previous workers. Normal serum was found in some cases to introduce artefact reactions, but these were eliminated by dilution. Relationships among the legumes were also indicated by gross anaphylaxis tests, and Inomata (132) used this reaction with success in showing the close relationship between lentils of various species. At about the same time, Sturm (343) found that serum immune to pea extract reacts strongly with pea but not at all with *Sambucus* and *Adoxa*.

Magnus and Friedenthal in 1910 published a further study (191) contending that there is a definite serological similarity between the sexual cells (pollen) and the somatic cells of a given plant species. The "conglutination" reaction (Mez' reaction) gave favorable results in the experience of Sauli (309) in showing the affinities of species of Cruciferae and Leguminosae. Precipitating sera for the sunflower were found by Galli-Valerio and Bornand (98) to react most strongly with sunflower extracts, less strongly with *Aster*, *Cynara*, artichoke, and non-Compositae in the order named. The same workers also showed (99) that precipitating sera for the poisonous *Amanita muscaria* react positively with *Amanita* extracts but not with those of species of various other fungus genera.

The agglutination test was used by Rosenblat-Lichtenstein (300, 301) in the hope of differentiating unicellular algae, and while her relationship tests are not of much significance because of the doubtful systematics of the forms used, her work is of interest in showing that green and colorless cultures of the same alga are serologically distinct, a finding which was later confirmed by Lieske (176), who further eliminated the weaknesses in Rosenblat-Lichtenstein's work, and showed that the agglutination reaction may be used with success in the differentiation of algae, since a given anti-alga serum agglutinates its homologous alga most strongly, closely related species less strongly, and widely separated species not at all.

Finally, in 1914 Zade (385) succeeded by means of the precipitin test in plotting a genealogical scheme of numerous species and varieties of *Avena* and of *Triticum*, which relationships were found to agree well with the existing views with regard to the systematics of the forms treated, both from the standpoint of their morphology and from that of their susceptibilities to certain parasitic fungi (Vavilov, 355). These early sero-systematic studies were reviewed by Janchen in two historical accounts in 1912 and 1913 (136, 137).

We thus see that prior to the studies of the Königsberg school abundant evidence had been brought forward that the serological tests, particularly the precipitin test, afford results entirely in keeping with the earlier systematics of the isolated plant groups tested, that immune sera in general react strongly with homologous plant antigens, proportionately less strongly with antigens from more or less closely related species, and not at all with very distantly related ones. Up to this time, however, the work had been limited to very few plant groups, and it accordingly remained for Mez and his associates in Königsberg to attempt an extension of this work covering the whole plant kingdom, correlating the results previously obtained, and greatly extending them to include many plant groups heretofore not investigated.

*The Königsberg serological "Stammbaum" of plants.* Under the leadership of Mez the precipitin and "conglutinin" (Mez'

TABLE 1

*Serodiagnostic publications of the Königsberg school, and subsequent study by Berlin workers*

| | KÖNIGSBERG | BERLIN |
|---|---|---|
| I. *Sero-systematic studies* | | |
| Plant kingdom, main outlines | Gohlke 1913 (104)<br>Mez and Gohlke 1914 (217) | |
| Archegoniates, main outlines | Guttmann 1924 (117) | |
| Algae | Steinecke 1925 (333)<br>Wilke 1929 (378)<br>Landfester 1933 (169) | |
| Fungi | Neuhoff and Ziegenspeck 1925 (254) | |
| Bryophytes | Mielinski 1926 (225)<br>Stepputat 1929 (335) | |
| Pteridophytes | Conradi 1926 (58)<br>Grenda 1926 (115)<br>Wilkoewitz 1929 (379) | |
| Gymnosperms | Mez and Kirstein 1920 (218)<br>Kirstein 1922 (146)<br>Mischke 1925 (228) | Eisenträger 1928 (74) |
| Monocotyledons | Worseck 1922 (383)<br>Ankermann 1927 (6) | Franz 1928 (96) |
| Dicotyledons | | |
| Ranales branch | Mez and Lange 1914 (219)<br>Lange 1924 (172)<br>Saltzmann 1924 (304) | Zarnack 1927 (387) |
| Rosales branch | Kohz 1923 (151)<br>Saltzmann 1924 (304)<br>Raeder 1930 (296) | Nay 1927 (248)<br>Schwandt 1930 (325) |
| Centrosperm branch | Malligson 1922 (192)<br>Bitzek 1928 (31) | Helwig 1927 (123)<br>Müller 1932 (242) |
| Parietales branch | Mez and Preuss 1914 (220)<br>Reuter 1926 (292) | Wermund 1928 (371)<br>Görner 1929 (105) |
| Columnifer branch | Hoeffgen 1922 (129)<br>Ruff 1930 (302) | Bärner 1927 (17)<br>Bry 1930 (38)<br>Hoepner 1930 (130) |
| Sympetalae branch | Alexnat 1922 (4) | Huhn 1927 (131)<br>Blass 1930 (35)<br>Scholz 1930 (318) |
| Miscellaneous dicotyledons | Raeder 1924 (286)<br>(Contested positions of such forms as: *Adoxa*, *Polygala*, *Arctostaphylos*, *Empetrum*, *Hypericum*, *Viscum*) | Zalkowitz 1932 (386)<br>(Sarraceniaceae, Nepenthaceae, Droseraceae) |
| II. *Published editions of the Königsberg Stammbaum* | Gohlke 1913 (104)<br>Mez 1922 (210)<br>Mez and Ziegenspeck 1926 (222)<br>Gortner, R. A. "Outlines of Biochemistry" (*N. Y.*) | |

TABLE 1—*Concluded*

| | KÖNIGSBERG | BERLIN |
|---|---|---|
| III. *Works correlating the Stammbaum with other systematic disciplines* | Ziegenspeck 1925 (389) (With anatomy)<br>Ziegenspeck 1925 (388) (With palaeontology)<br>Ziegenspeck 1927 (391)<br>Mez 1936 (261b) (With morphology)<br>Mez 1928 (216a) (With cytology) | |
| IV. *Techniques and theories* | | |
| General | Mez 1922 (210), 1924 (211), 1926 (213, 214, 215, 216)<br>Mez and Ziegenspeck 1925 (221), 1926 (222)<br>Ziegenspeck 1926 (390)<br>Steinecke (= Mez) 1925 (212)<br>Steinecke 1925 (334) | Gilg and Schürhoff 1926 (101)<br>Gilg and Schürhoff 1927 (102, 103) |
| Use of various tissues | Wilkoewitz and Ziegenspeck 1928 (380) | Burger 1929 (39)<br>Wartenberg 1930 (359) |
| Artificial sera | Mez and Ziegenspeck 1925 (221) | Sasse 1928 (308)<br>Nahmmacher 1929 (245) |
| Function of lipoids, and other artefact reactions | Becker 1932 (22) | Meyer 1929 (209) |
| Use of purified proteins | | Arms 1928 (7) |

reaction) reactions have been applied to a great many species of plants representing most of the plant families from bacteria upwards. The results have been gathered in the form of a genealogical tree of plant relationships, or serological "Stammbaum." The work commenced with a blocking out of the main serological subdivisions of the plant kingdom by Gohlke (104), and then each subdivision was made the subject of intensive research by one or more collaborators. For the purpose of ready reference to an otherwise rather complex literature, Table 1 provides references to studies of the various taxonomic units. It is to be regretted that the complete Stammbaum has been patented and hence cannot be reproduced here in its entirety, but Table 1 includes references to its published editions.

In an earlier part of the present paper the techniques of the Königsberg investigators have been dealt with in detail, and it is hence unnecessary to consider them here. A word should be said, however, as to the method of construction of Stammbaum branches.

The customary procedure in Königsberg consists in the selection of a number of representatives of the group to be studied, an attempt being made to select forms representing as many important subdivisions of the taxonomic scheme as possible. Each of these is used for immunisation, and the reactions obtained when such sera are tested against many kindred species are plotted in the form of circles of greater or less radius according to strength of reaction, and concentric to the serum, which represents a "reaction centrum". By observing the degree of intersection of circles from two centra, it then becomes possible to orient a test species with regard to the two centra involved. More centra and more test species are added until the whole forms a logical system of coordinated relationships.

During the development of the Königsberg work new techniques came to replace

older ones, and hence it became desirable to re-test the groups studied earlier; an examination of the table will show that as a rule the relationships of each group have been confirmed at least once in comparatively recent times (the Königsberg techniques reached essentially their present form in 1924–1926). It will be recalled that Mez considered as significant only results which have been reciprocally tested by both precipitin and Mez' reactions, in which the controls are faultless, and in which the investigator has performed and evaluated all reactions without knowing the identity of his test extracts.

It is highly significant that, although the studies of the Königsberg workers have frequently overlapped, the cases of disagreement are comparatively rare. One such case was that of *Ginkgo* which was erroneously misplaced in an early study. Mez' critics have made much of this relatively isolated case, often losing sight of the fact that the error was discovered and rectified in Mez' laboratory before it had been commented on by outside investigators.

Mez lays considerable stress on the qualification that, besides being purely objective and faultless in execution, the method must produce results which form a logical systematic arrangement. There is no question that the Stammbaum as published in 1926 (222) represents such an achievement. The work has excited both favorable and adverse criticism, the critics forming three groups, viz. plant systematists such as von Wettstein, Diels, Stolley, and Heintze who have viewed the work purely from its systematic results, the Berlin-Dahlem workers associated with Gilg and Schürhoff who have attempted a re-testing of the Königsberg Stammbaum by the use of modified serological methods, and a number of independent critics, as for example Boom, Moritz, Kōketsu and Kojima, Krohn, and Hannig and Slatmann, who have also separately re-tested taxonomic units of plants either by the Königsberg techniques or by modifications of or supplements to these.

As regards the criticisms of plant systematists, there is on the whole a tendency for these to regard the Königsberg system as a stimulating attack on many old controversial taxonomic problems. It is not unnatural for the serological Stammbaum to be looked on with some suspicion wherever the results do not strictly conform with the opinions of the critic in question and for it to be accepted uncritically as an excellent confirmation whenever it does substantiate the opinions of systematists. Systematics is so frequently based on opinion rather than on objective demonstration, that it is to be expected that different systematists should differ in their taxonomic alignments, and under the circumstances no such system as the Königsberg tree could be hoped to satisfy all opinions, many of which are conflicting or diametrically opposed. In all the important relationships of the Königsberg tree which have been criticized by certain systematists, Mez has shown that other systematists agree with the relationships detected serologically. Moreover, in each of the recent Königsberg studies, the group investigated has been worked over from the standpoint of morphology as well as of serology, and the same logical sequences have arisen from both disciplines. The attack on the Königsberg work by the Berlin investigators also has had its effect on the acceptance of the Königsberg Stammbaum by systematists, the latter being unversed in serological techniques and often accepting as just criticism the not wholly unprejudiced and sometimes

intemperate contradictions of the Berlin school. A consequence of the unfortunate controversy between the two schools has been to impair the repute of the serosystematic method, although the more impartial and soundly scientific attitude of such recent workers as Boom in Holland, Moritz in Kiel, and Krohn in Finland have fortunately had a stabilizing effect on the disturbed state of serosystematics in Europe. The reception of the Königsberg Stammbaum by plant systematists of the older school may be illustrated by the following representative cases.

Von Wettstein (372) has expressed the view that serology gives systematic results well worthy of consideration by systematists. The methods are held to be still in a relatively primitive state, and accordingly von Wettstein felt that serodiagnosis by itself cannot be considered decisive, but must be controlled by comparative morphology. The chemist, Molisch (228a), and the animal serologist, Boyden (36a), have come to similar conclusions. Stolley (337, 338, 339) in a series of rather vituperative notes has bitterly criticized the Stammbaum which in certain cases fails to accord with Stolley's views as to the phylogeny of the forms in question. Mez has shown, however, that with regard to the systematics of the disputed forms Stolley stands alone in his opinions, opposed not only to the Königsberg findings but also to the contentions of systematists in general. For example, the Stammbaum, in accordance with the views of nearly all systematists, indicates that the mosses preceded the Pteridophytes in origin, while Stolley objects to the Stammbaum because of this. Heintze in Sweden finds that "serodiagnostic investigations have hardly contributed to a clearing up of the relationships within the Cormophytes. By and large, they only 'confirm' the errors of Engler and other authors" (121). But this serves to illustrate again that the discrepancies in opinion among the systematists themselves are such that it becomes impossible for any system to meet with the approval of all. Diels in Berlin (67) feels that the techniques at present are relatively inadequate but that it is entirely possible that the serological methods may in time come to afford very considerable aid to systematics.

Thus, on the one hand, we find the Königsberg Stammbaum accepted only with considerable reservation by many systematists. On the other hand, Mez maintains that the serological system is more fundamental and reliable than others based on less empirical criteria. He bases his position on his contentions that: (1) serology is empirical while most of the other approaches to systematics are necessarily based on opinion, (2) serology deals with the relationships of the species-specific proteins, presumably of the proteins of the chromatin, while morphology deals with far removed consequences of chromatin activity, and (3) morphological systems show frequent unnatural "convergences of evolution" while these have never been seen in serological studies. However, Mez' original position has been conservatively stated:

> "With emphasis I take Janchen's standpoint that the serum reaction in itself asserts nothing regarding relationships but only teaches one regarding protein similarities, that is, physiological-chemical agreements and discrepancies. Under no conditions have we in the serum reaction a systematic panacea, but we see a new method of investigation open, the results of which must be compared with other systematic methods, and not considered as more valuable, but only as equally deserving of consideration in the balance." (Transl. from 217.)

With the more complete development of the method, Mez has come to place

more and more reliance on it because of its value as seen in an empirical way. Many doubtful and controversial relationships have yielded satisfactorily to the serological method (222).

On the whole, the results of the Königsberg investigations seem to be highly deserving of consideration from a systematic viewpoint. Theoretically the method appears more fundamental than any other systematic approach; practically this is limited by the complexity of the technique and the not infrequent occurrence of artefact disturbances. Yet with the advance of phytoserological techniques these artefacts are coming more and more under control, and it is noteworthy that even the results of the antagonistic Berlin school, in spite of numerous reported systematic discrepancies, nevertheless include many findings wholly in support of the Königsberg Stammbaum (e.g. 17).

*The Berlin "refutation" of the Königsberg "Stammbaum."* In 1926 a group of students working in association with Gilg and Schürhoff in Berlin-Dahlem commenced a series of studies ostensibly for the purpose of re-testing the correctness and value of the Königsberg Stammbaum. Each of the main branches of the Spermatophytes was re-worked by one or more investigators, and in addition a number of studies were undertaken for the purpose of testing techniques as well as results. Table I shows in concise form the scope of these studies.

It is indeed remarkable that, while thirty investigators at Königsberg all found the serological methods suitable for the study of plant relationships, a score of workers at Berlin have nearly all come to opposite conclusions, stated in the following terms of Gilg and Schürhoff:

"Die Serodiagnostic ist für die botanische Verwandtschaftsforschung völlig unbrauchbar. Wir sind der Meinung, dass schon diese einzige Ueberlegung vollkommen genügt, um zu zeigen, das man an den Ausfall der unspezifischen Reaktionen keine weitgehenden Deduktionen anschliessen darf" (102). To be sure, this dogmatic position is somewhat qualified by certain of the Berlin workers. Blass, for example, while recording a number of apparent contradictions in his tests, still considers the method helpful in phylogeny, although he does not feel that a Stammbaum can be erected on such reactions alone. He, like certain other Berlin workers, has drawn rather far-reaching conclusions from his serological tests in spite of his refutation of the method. ("Out of the totality of my relationship reactions, especially of the behavior of the Cucurbitaceae and Umbelliferae, I believe it very apparently probable that the division of the Dicotyledons into the two classes Choripetalae and Sympetalae no longer corresponds to the true genetic relationship of the families classified thus.") (Transl. from 35.)

All of the Berlin investigators find serology satisfactory for the identification of plant antigens, while doubtful of its value in relationship study.

That two large groups of investigators working in the same field could come to such contradictory conclusions is a paradox demanding interpretation. Gilg and Schürhoff contend that the Königsberg Stammbaum is not purely objective but that it has been influenced by the preconceived opinions of the Königsberg workers. Mez and in addition certain independent workers, notably Boom, maintain that the Berlin work is not free from prejudice. Assuming that neither contention is correct, that the work of both schools is purely objective, there still remain differences in the methods of the two groups sufficient, in the writer's opinion, to account for this discrepancy. These differences in methods concern the execution of the tests and the evaluation of the results, the latter probably being most important in the present connection.

The techniques of testing differ materially in the two schools.

In Königsberg the precipitin flocculation test and Mez' reaction are used in parallel, while in Berlin neither has been adopted, and instead the precipitin ring test is the chief reaction. In Königsberg it is customary to preëxtract all antigenic materials with alcohol, while in a large part of the Berlin work this preëxtraction has been avoided because of an assumption that alcohol preëxtraction will denaturate the antigens. The period of incubation of precipitin mixtures is 12 hours at 37°C. in Königsberg, and from 10 minutes to about 1 hour at room temperature in Berlin. The serum titers in Königsberg are relatively high, of the order of 1:25,000 or 1:50,000 in many cases, while in Berlin they are more frequently of the order of 1:1000.

Mez' rigid system of controls has been described in a previous section. Emphasis should be laid on the requirement that reactions must be reciprocally confirmed before they are incorporated into the relationship studies, and that any experiment must be rejected in which the controls, particularly the tubes containing antigen + normal serum, are faulty.

The Berlin objections to the Königsberg results are based chiefly on the observations in Berlin that normal serum controls are often faulty, that non-specific positive reactions frequently appear, connecting distinctly unrelated species, while closely related forms are not infrequently negative with one another, and that the reciprocal reactions often do not confirm one another.

According to the Königsberg incubation schedule, normal serum frequently precipitates in the presence of various antigenic extracts. This is particularly true if the antigens are not preëxtracted with alcohol. The occurrence of such normal serum precipitates, which can be greatly lessened by adequate preëxtraction and a shortened incubation time, and which can be entirely eliminated by absorbing all extracts with normal serum (152, 153, 154) or in certain cases at least by extracting in phosphate buffers (118, 119, 330), in no case serves as a valid objection to the Königsberg work if for no other reason than that all Königsberg experiments in which normal serum cloudings appear are rejected. That the Berlin workers often obtain negative results with closely related forms is doubtless due to the low titer reactions, the ring test observed after 10 to 60 minutes not being as sensitive as the flocculation test, even though the incubation time for the latter is materially shortened. The positive non-specific reactions of the Berlin workers in many cases are due to an insufficient preëxtraction of the antigens thus failing to remove such non-specific precipitating substances as the lipoids. Mez also uses a variety of techniques for removing tannins, glucosides, alkaloids, organic acids, gums, resins, and other substances which may produce non-specific results, and such techniques have rarely been incorporated into the Berlin work.

Furthermore, the occurrence of an isolated non-specific reaction, even if due to an unrecognized variable, need not prove detrimental to serosystematic work since its presence will usually be detected at once by the fact that the reciprocal tests involving it will not harmonize, and hence, according to Mez' requirements, reactions with such an antigen will be automatically discarded.

We have, then, two distinct types of reactions to consider, the specific protein reactions which are found by both schools to harmonize with systematic conceptions, and non-specific reactions which lead to impossible relationships. Both types of reactions have been obtained by both schools of workers. But here lies the essential difference in viewpoint. The non-specific reactions in large part are removed by the Königsberg techniques and those which remain become eliminated under the rigid qualifications of the Königsberg control system. There remains a residuum of reactions which form a

logical, coördinated taxonomic system. In Berlin, on the other hand, the non-specific reactions are more frequently obtained than in Königsberg because of the excessive caution in preëxtraction technique; in general they are not automatically eliminated by reciprocity requirements; and finally the emphasis on them in interpretation tends to overshadow the truly specific relationship reactions obtained so frequently in Berlin. Herein, in the writer's opinion, lies the basis for the misunderstanding between the two schools, a misunderstanding not necessarily resting on a subjective coloring of the results in either case, but upon such differences in technique and in control requirements that the works of the two are hardly comparable. In no case is it to be concluded that the Berlin work constitutes a decisive refutation of that of the Königsberg school.

*Re-testing of the Königsberg studies by independent investigators.* The Königsberg work excited immediate attention among systematists and as early as 1917 Kōketsu 153, 154) and Kojima (152) in Japan proceeded to test the value of the serological techniques on a broad scale. The precipitin tests showed the Dicotyledons to be widely separated from the Gymnosperms in systematic position with the Cycads connecting the Gymnosperms to the Dicotyledons through *Magnolia*. Within the Gymnosperms the results in general agree with those of modern systematics, although with a few deviations. The work of Kōketsu and Kojima is of particular interest inasmuch as they completely eliminated the normal serum artefact reactions by absorbing all extracts with normal serum before testing. The isolated non-specific reactions obtained may well be due to the excessive presence of tannins in the Gymnosperms, no mention being made of their removal, as well as to the fact that reciprocal confirmation was not held to be an essential qualification for the acceptance of a result as valid.

Arzt in Leipzig (8) applied the precipitin technique to a study of the relationships of the Gramineae. While his work in some respects seems to lack sufficient background in the essentials for serological research, the results appear to be valid. His work indicated that barley is nearer to oats than to wheat, and that among the barleys there are minor serological differences, the latter, however, being rather weak and not entirely convincing, in accordance with the failure of most other investigators to separate varieties of a species by the precipitin test.

The greatest weakness of the Königsberg methods appears to be the reliance on the value of Mez' and Ziegenspeck's artificial sera in the most recent Königsberg work.

Sasse (308) and Nahmmacher (245) in Berlin pointed this out in 1928 and 1929, respectively, and in 1928 Grijns (116) in Holland and Eisler (75) in Germany also came to the conclusion that this feature of the Königsberg technique is not reliable. Boom in Holland (36) has likewise arrived at the same conclusion. Boom's work represents one of the most satisfactory attempts at a re-testing of the serodiagnostic method. He conservatively accredits the value of the serological Stammbaum while emphasizing the need for morphological control of its results. So many factors influence the strength of serological tests that in general they are considered of value chiefly from their qualitative rather than their quantitative features. This fact has been emphasized by Boom in connection with his sero-systematic studies, and serves as a major limitation to the serological approach to plant systematics.

An interesting contribution was made in 1928 by Hannig and Slatmann (330, 118, 119). They found that the addition of phosphate to the extractant serves to eliminate the normal serum artefact reactions so frequently observed especially in Berlin, although there may be some question whether this technique does not in itself introduce artefact reactions of another sort. Thus, although they obtained some results bearing out systematic concepts, numerous non-specific reactions were seen, and from their tables of reactions it appears that in many cases these excep-

tions were of combinations in which the reaction was properly negative until phosphate was added.

In 1931 Silberschmidt (327) analyzed the Berlin-Königsberg controversy and pointed out in particular the necessity for adequate preëxtraction of extracts, constancy of electrolyte constitution of extract dilutions, a complete control system, the avoidance of the zone phenomenon by the proper adjustment of proportions of antigen and serum, and a continuation of the observation time beyond the interval usually used in Berlin. Jaretzki (138) in a review of the subject the same year concluded that the value of the serological method in systematics consists chiefly in a confirmation of the deductions from other disciplines and in a stimulus to re-investigation in those cases in which serology fails to accord with morphological findings.

Moritz in Kiel has made a valuable contribution in his analysis of the value and limitations of the Königsberg work (230, 231, 232, 235) and in his development, with vom Berg, of the anaphylaxis (Schultz-Dale) technique as an aid in sero-systematics (233, 234, 236, 237, 239, 24). His criticism particularly regards the facts as pointed out that the Berlin technique is much less sensitive than that in Königsberg, and that the Berlin preëxtraction is inadequate and leads to artefact reactions. The Königsberg results are considered error-free provided that experiments in which the normal serum controls are faulty are rejected (a Königsberg requirement). His experiments with Mez' artificial sera, like those of Sasse, Nahmmacher, Boom, Eisler, and Grijns, were entirely unsuccessful.

An important feature of Moritz' contribution lies in his analysis of the plant antigen in terms of complexes of single antigen units or partigens. The relationship reaction is interpreted as a mosaic of identity reactions of such partigens, the strength of the total reaction depending on the frequency and concentration of common partigens and partigen-antibodies. This conception is elaborated and illustrated by means of Moritz' and vom Berg's anaphylaxis tests which will be mentioned subsequently.

Krohn in Finland (162) has recently published an excellent study on the serological affinities of the Sympetalae branch of the Königsberg Stammbaum. This author first shows that the Berlin repetitions of the Königsberg work in this group are supported by data which are entirely inadequate and in which, because of numerous faulty control reactions, the results obtained are neither significant nor comparable with those of Gohlke and Alexnat in Königsberg. He then proceeds to investigate the group serologically in elaborate detail and following precisely the techniques of Mez. The results obtained offer a detailed confirmation of the Königsberg results with this group and show at once the objectivity of the Königsberg approach and the inadequacy of the Berlin "refutation". An important contribution is Krohn's extensive data showing that a single species may be taken as representative of its family, serologically.

*Sero-systematics of animals.* Studies analogous to those of Mez have been executed for the purpose of determining the serological relationships of animals. The classic introductory work of Nuttall has already been mentioned. Subsequent workers have published accounts of the serological relationships of crustacea, lepidoptera, helminths, amphibia, birds, cattle, and other mammals. The literature concerned has been adequately reviewed by Boyden (36a).

*Isolated sero-systematic investigations.* In 1923 Rives (294, 295) in France applied the precipitin technique to a number of varieties of the grape and found that a failure to graft successfully was accompanied by a marked serological difference, while varieties which grafted successfully were similar according to the test. A similar type of work was undertaken by Green in 1926 (114), in an attempt to study the possible correlation between serological affinity and grafting compatibility in *Citrus*, Rosaceae, and Solanaceae, and the precipitin test showed that such forms as grafted readily reacted positively, and that poor graft combinations were more distantly related serologically.

Maschmeier in 1927 (196) was unable to separate varieties of the potato by precipitin reactions according to the Berlin methods, in accordance with the failure experienced by other workers in the separation of such closely related forms. It is now known, however, that the latent mosaic virus of potato is present in practically all potatoes in the field and that potatoes infected with this virus

(which may cause no symptoms of disease) give a strong precipitin test specific for the virus. Hence, Maschmeier's failure to separate potato viruses by the precipitin test may be due in part at least to the strong virus reaction, which would tend to obscure specific potato protein reactions. Kato and Maruyama (144) likewise failed in the separation of rice varieties by ordinary methods, although a saturation technique afforded a clear cut differentiation of Japanese, Chinese, Korean, and Formosan rices. Moreover, Nelson and Dworak (249, 251) have reported experiments in which it was possible to separate wilt-resistant from wilt-susceptible flax varieties by using the seed globulins in precipitin tests, although it was impossible to do so if extracts of the whole seeds were used, implying that the varietal-specificity of the precipitin reaction with these seeds is confined to the seed globulins and masked by reactions of other proteins common to all varieties of the species. The writer, in attempting to differentiate genera and species of the Solanaceae by the Schultz-Dale anaphylactic reaction, has also found that the globulins of the leaves of these plants show much more specificity serologically than do the albumins or the total-protein extracts (unpublished).

A very interesting aspect of serosystematics is brought out in the work of Baldwin, Fred, and Hastings who in 1927 (13) showed that the serological reactions of numerous legumes bear a strong and consistent relationship to the differential susceptibilities of these legumes to various strains of nodule bacteria. Precipitin and anaphylaxis reactions were used with comparable results, and it was found that all groups of legumes inoculable with the same nodule bacteria were serologically homogeneous, and that serologically similar groups were nearly always cross-inoculable with the same bacteria. No serological affinity was found between the legume seed-proteins and the proteins of the bacteria infecting the same legumes.

According to Elmore (80, 81), the complement fixation test satisfactorily distinguishes *Euglena*, *Chlorella*, and *Chlamydomonas*, and, while different green strains of *Euglena* were not separable, green and colorless forms of the same strain were distinguishable (cf. Rosenblat-Lichtenstein, 301, and Lieske, 176). The complement fixation test also serves to differentiate *Aspergillus niger* from *A. fumigatus* in the experience of Corpaci (61), although the agglutination and precipitin tests proved unsatisfactory. Matsumoto (197, 198) had a similar experience using 23 species and strains of *Aspergillus*, the homologous reactions always being strongest, and heterologous combinations reacting more weakly. *Gloeosporium* and *Penicillium* were found to be too distantly related to react with *Aspergillus*. The method was accordingly recommended for the differentiation and identification of fungi. Similarly, the precipitin test served for a satisfactory differentiation of some yeast species, although the complement fixation test is felt desirable in this connection (266). Using her "cytotoxin" test (see p. 42), Sauer has recently shown that *Euglena gracilis* strains may be serologically separated into a group showing luxuriant growth and a group with delicate growth (308a). Green and colorless forms of the same alga were inseparable by this method, although Sauer earlier reported (80, 81) a serological difference between such strains.

In 1928 the complement fixation technique was used by Coons and Strong (60, 59) in a study of *Fusarium* species, and while the method was considered too laborious and exacting for general appli-

cation, its value in special investigations was indicated. Thus, *F. radicicola* and *F. Martii* were readily distinguished from each other and both from *F. conglutinans*, and in one case even a varietal distinction was possible. Link and his associates likewise have been carrying on work in the serological differentiation of *Fusarium* and other fungi (180, 181). While the *Fusarium* species showed a strong common group reaction, various genera of the Pezizales were separated from the Hypocreales, and certain of the latter from one another. A number of difficulties were encountered, some homologous tests being constantly negative and some group reactions being so extensive as to preclude satisfactory genus and species differentiation, and Link's recent work on the serology of the fungus carbohydrates (376) indicates that the serological activities of these substances may be a cause of this latter difficulty. Beck in Canada (21) also had certain difficulties in the separation of Ustilaginaceae by the precipitin test, but both Link and Beck indicate that the method, although limited with respect to the fungi, offers possibilities in the determination of affinities among species and genera of doubtful systematic relationships. Canonici (41) did not find the serological tests as delicate as biochemical tests in the differentiation of 209 strains of *Aspergillus niger*, although positive group reactions were obtained with both the precipitin and agglutination methods.

In America the higher plants have also been studied serologically. In 1929 Nelson and Birkeland (250), using Mez' techniques and absorption precipitin tests on the globulins of wheats, found the serological characterization of the wheats studied to be correlated to a certain extent with such genetic characteristics as resistance to stem rust and yield, so that within limits the serological technique may be used as an aid to the plant breeder in the early orientation of genetic variants with respect to their genetic characters. In a similar but more extensive study in 1931, Edgecombe (71) found a close parallel between the wheat globulin precipitin test and relationships of wheats as measured by their resistance or susceptibility to *Puccinia*, although the chromosome numbers did not agree with either of the other criteria.

Thus, taken as a whole the serological approach to plant systematics as first elaborated in Königsberg has been supported, although qualified to some extent, by numerous independent investigations, the inadequacies of the Berlin "refutation" have been made evident, and the net result indicates that serosystematics is on a sound basis provided that rigorous care be taken to exclude non-specific reactions. Some methods of such exclusion have been pointed out. The whole subject of serology is still in an imperfect state and this applies in particular to that of the complex plant antigens. But even with this qualification, emphasizing as it does the need for a further development of the phytoserological techniques, from a purely empirical standpoint serology has proven itself of value not merely as an adjunct to other systematic procedures, but as an approach with advantages appertaining to no other systematic method, and with disadvantages which are rapidly becoming minimized with the advance of technical knowledge.

*The sero-systematics of the plant viruses.* Beale has shown (279, 280, 281) that tobacco mosaic plants possess precipitinogenic properties apart from those of healthy host plants, and this has been repeatedly confirmed. Dvorak (70) noted that a similar situation exists with regard to mosaic potato plants, a fact which has been confirmed by Gratia (106, 108–110) and others. Beale likewise observed that tobacco mosaic immune serum fails to precipitate

in the presence of extracts of plants containing Sudan grass mosaic, Hippeastrum mosaic, lily mosaic, Abutilon mosaic, peach yellows (281), and mosaics of *Asclepias*, *Datura*, and *Solanum* (283). In none of these cases, however, were immune sera prepared with respect to these heterologous viruses in order to demonstrate a true cross-specificity of the tobacco mosaic reaction. Gratia (106, 108–110, 112), however, showed that tobacco mosaic serum precipitates tobacco mosaic extract but not a potato mosaic extract, and that, reciprocally, potato mosaic serum precipitates potato mosaic extracts but not tobacco mosaic extracts. He was unable to prepare a serum reactive with the veinbanding virus of potato.

The question of specificity was carried somewhat farther by Birkeland (29) who found that tobacco mosaic extract, cucumber mosaic extract, and "spot necrosis" extract are each precipitated by its specific antiserum but not by the other two heterologous antisera. Precipitating sera for Wingard's tobacco ring spot were not obtained. Spooner and Bawden have recently confirmed Birkeland's work in part (331). Manil in a survey of tobacco leaf necroses (193a) found that several types of such disease were serologically related to tobacco mosaic, although the majority of plants tested failed to react with either tobacco mosaic or latent potato mosaic virus sera. Recently Verplancke in Belgium (356, 356a) has claimed to have prepared a precipitating serum for a sugar beet mosaic which was negative against various other virus materials, and Matsumoto (201a) has been able to distinguish two similar-appearing *Petunia* mosaics serologically, since saps of one type showed a tobacco mosaic reaction, while the other type was serologically inactive.

The writer has immunized rabbits with extracts of tobacco mosaic, cucumber mosaic, and tobacco ring spot, and obtained sera from each which specifically neutralized its homologous virus but neither of the two heterologous viruses in each case (47). Continuing and extending this earlier work, attempts were next made to utilize the serological tests for the purpose of aiding in the study of the classification of the plant viruses. The results have been encouraging and may be briefly summarized as follows (49, 50, 54):

Up to the present 60 viruses and virus strains have been tested, and about half of these give specific serological tests. The reactive viruses may be classified in 8 groups. It may be stated parenthetically that this step toward classification rests in largest part on precipitin tests, and that in nearly every case the precipitin tests have been reciprocally confirmed by using each virus both as antigen for serum preparation and for testing, respectively.

1. *Tobacco mosaic group.* This group comprises field-type tobacco mosaic, Holmes' attenuated and symptomless strains of tobacco mosaic, Jensen's yellow and necrotic tobacco mosaic isolants, aucuba mosaic of tobacco, and Johnson's tobacco virus VI. It may be noted here that the serological evidence regarding this large but uniform group of tobacco mosaic segregates is in entire conformity with their homogeneity as shown by thermal death point, infectivity, host range, and other characters so far as these have been studied. According to the tests of Manil (193a) and Matsumoto (201a), certain Belgian necrotic diseases of tobacco and a Japanese *Petunia* mosaic or components of these also belong in this group.
2. *Latent potato mosaic group.* This group includes the potato ring spot and spot necrosis (or a constituent of the latter if it is a mixture) used by Birkeland, as well as the attenuated spot necrosis employed by him. It includes also the typical latent virus of potato (X-virus and D-virus (331) of potato in the European literature), Hyoscyamus IV virus (112), potato mottle, and British Queen streak. All of these viruses show a greater or less necrosis of the ring spot type, but at least one, the potato mottle, represents a mixture of two or more latent virus strains, from which has been segregated one, a relatively non-necrotic, almost symptomless type, and another of the tobacco ring spot type, each of which behaves exactly like the others serologically. The viruses in the tobacco mosaic group are so closely related serologically that there is very good ground for considering all of the viruses of the group as strains of the same virus type, and the same may be said for the viruses of each of the other groups, respectively. Even the complement fixation reaction shows them to be remarkably uniform as groups. Spooner and Bawden (331) have recently found a number of strains of this group to be indis-

tinguishable by precipitin, complement fixation, and neutralization tests, the reactions being independent of solanaceous host species. Rugose mosaic of potato consists of a mixture of a strain of the veinbanding virus + any strain of latent mosaic.

3. *Potato veinbanding virus* (Y-virus of potato in the European literature) and cucumber mosaic. These two viruses have shown an unexpectedly close serological relationship, so close that they are considered strains of the same virus type. This finding is corroborated by their similarity in infection of the cowpea (*Vigna sinensis*). Valleau's delphinium virus 10729 also belongs in this group.
4. *Aucuba mosaic of potato.*
5. *Mild mosaic of potato.* (Reaction weak.)
6. *Tomato etch and severe etch.* These two viruses are evidently strains of the same virus type as shown by their cross precipitin tests, and are distinct from the other groups here listed. On one or two occasions cross-reactions with tobacco mosaic virus juice were obtained, but these were probably due to a contamination of the material, as later tests have shown the etches to be entirely distinct from tobacco mosaic.
7. *Wingard's tobacco ring spot* (green and yellow strains).
8. *Osborn's pea mosaic viruses #2 and #3*
9. Verplancke (356) has recently reported positive precipitin tests with serum and extracts of *beet mosaic*. His serum was negative toward extracts of beets with yellows, of mosaic *Dahlia*, of potato with mottle, crinkle mosaic, mild mosaic, streak, streak mosaic, and leaf roll, of mosaic *Pelargonium*, or of *Monstera* with *Anthurium* mosaic. It cannot be determined from Verplancke's scanty data whether or not this represents a distinct virus type from those enumerated above (beet mosaic is sometimes associated with cucumber mosaic in the literature). The question seems justified whether this worker actually was dealing with a virus reaction, since the data are meagre, no mention is made of healthy beet or normal serum controls, the incubation period was excessive (24 hours at 37°), dilution series were not employed, and the serum precipitated beet mosaic extract which had been heated nearly to boiling, in contrast with the findings regarding all other viruses heretofore tested (51). Gratia and Manil (113) were unable to confirm this work of Verplancke's.

Each of these 8 groups of viruses is distinct serologically from the other groups. Within the tobacco mosaic group, the ordinary precipitin technique fails to show differences among the various strains of this virus, although recently it has been shown that a technique of precipitin absorption serves to reveal minor serological differences among the strains of tobacco mosaic virus (54). The same has been shown for the latent potato mosaic group.

We thus see that a fair start has been made toward a study of the relationships of the plant viruses by serological methods; the results obtained are in harmony with the results of other approaches to the question of virus classification, and serology accordingly bids fair to be a useful and impartial aid in this difficult field. As has been pointed out, however, (50) certain viruses, notably those transmitted with difficulty or not transmissible mechanically, have not yet proved good material for serological study. Thus Gratia and Manil were unable to prepare satisfactory antisera for the viruses of potato veinbanding, beet mosaic, and sweet clover mosaic (113), Beale failed to obtain sera for a number of viruses, and the writer could not obtain precipitating sera for the viruses of peach yellows, aster yellows, and certain other diseases. In general, the viruses which have thus far proved amenable to serological examination have been those which are readily transmissible by mechanical means, and which are relatively resistant to physical and chemical agencies. It is to be hoped, however, that improvement of the techniques of handling viruses *in vitro* will extend the present limit of application of serological methods to the plant viruses.

### III. SEROLOGY OF PURIFIED OR ALTERED PLANT PROTEINS

*Investigations of Wells, Osborne, and their associates.* Osborne's notable success in

the purification and crystallization of many plant proteins at a time when relatively few animal proteins were obtainable in a comparable state of purity (263) quite naturally led to a particular interest in the serology of such preparations. In coöperation with Osborne, Wells, Jones, White, Avery, Lewis, and others have carried out intensive studies in this field, combining a study of the serology of the purified proteins with investigations of their chemical nature and differentiation.

The various techniques employed consisted of the gross-allergy test, precipitin reaction, complement fixation reaction, Schultz-Dale anaphylaxis test, bronchospasm and skin anaphylaxis reactions, and the Abderhalden reaction. The results of all these tests were in general agreement, the Schultz-Dale and bronchospasm tests proving most sensitive and specific, the precipitin, complement fixation, and gross-allergy tests satisfactory but less delicate, and the Abderhalden reaction on the whole in conformity with the other reactions but more subject to aspecific, distorting results.

In general, all of the purified plant proteins are antigenic, but they differ considerably in their antigenic power. Those which are relatively insoluble in the body fluids (e.g. the alcohol-soluble zein), show a tendency to be least active antigenically (361). The plant proteins show a degree of reactivity entirely comparable with animal antigens, and the titers obtained are often relatively high, e.g. complement fixation to 1:1,000,000 with hordein, to 1:100,000 with wheat gliadin and squash globulin, precipitin reaction to 1:100,000 with edestin (168). In immunisation the serum first shows a positive complement fixation test, then in addition a precipitin reaction, and finally also the anaphylaxis reaction. Likewise, each reaction is sharply specific when it first appears, but as the titer increases reactions occur which involve proteins from species more or less closely related to that providing the immunisation antigen (168).

With regard to the serological relationships of the plant proteins, the following facts have been determined. Zein and gliadin are entirely distinct (361); the gliadins of wheat and rye are similar or identical, as are the legumins of pea and vetch, while vicilin is relatively close to the latter two (365). Gliadin of wheat and rye and hordein of barley are closely related but distinguishable; glutenin shows affinities to gliadin although serologically and chemically distinct (366). Edestin is entirely distinct from gliadin and both from the globulins (374). Within the wheat seed there are at least 5 proteins, globulin, gliadin, glutenin, leucosin, and natural proteose, all distinct serologically (382). The globulins from squash and canteloupe are serologically and chemically identical (140). The alcohol-soluble prolamines of wheat and its relatives all show serological relationship, as do those of corn and its relatives, although a relationship of the wheat-group prolamines with the corn-group prolamines is lacking (174, 175). It would thus seem that the Königsberg technique of alcohol preëxtraction may in some cases remove proteins of value in the relationship reaction. In the legumes also the serological reactions follow closely both the chemical constitution of the proteins and their botanical relationships (364).

We thus see that, while a single species of plant may contain several serologically and chemically distinct antigens, and while also two closely related species may contain chemically and serologically indistinguishable antigens, nevertheless, considering either the protein complex of the plant as a whole or single protein constituents of the complex, the serological reactions obtained by precipitin, anaphylaxis, and complement fixation techniques correspond closely to the results of chemical analysis so far as the latter is possible, and to the botanical relationships of the plants yielding such proteins (cf. 263). The often-repeated statement of Wells and Osborne that "the anaphylaxis reaction depends on the chemical structure of the protein molecule and not on its biological origin" (369) implies a contradiction which actually does not exist, since their own later work has shown adequately that serological relationships, chemical relationships, and botanical relationships of the purified plant proteins all go hand in hand. The plant sero-systematists have shown the correlation between serological affinity and botanical affinity of plants, and Wells and Osborne and their colleagues

have supplied the connecting link between these two: chemical relationship.

*Isolated researches with purified or altered plant proteins.* Previous to the first researches of Wells and Osborne on the serology of purified plant proteins, a few scattered attempts had been made to study the antigenicity of such proteins.

As early as 1901 and 1902, Schütze (320) and Castellani (43) independently showed that "Roborat", a hot-water-soluble derivative of legume protein (*Pharm. Ztg.* Oct. 2, 1900: 770), induces the production of precipitins which are specific in the sense that Roborat sera react against Roborat extract but not against various animal protein preparations.

In 1904 and 1906 (Obermeyer and Pick (261, 262) observed that an iodized or nitrified animal protein loses ability to react serologically with natural protein of the same source, but does react specifically with unrelated iodized or nitrified protein respectively. Tests with the plant protein edestin gave similar results. This work, which has since been repeatedly confirmed, gives evidence that the specificity of the protein molecule rests in relatively minor radicals of the molecule.

Rosenau and Anderson in 1908 (299) successfully employed crystalline edestin and excelsin in the anaphylactic sensitization of guinea pigs, although the results obtained were by no means as clear as those obtained by Wells and Osborne. This type of work was successfully repeated in Germany by Bürger in 1914 (40), who obtained good anaphylaxis reactions with native gliadin, zein, edestin, amandin, and excelsin, but no cross-specificity with acid digests of the same proteins. The serology of the crystalline protein associated with the mosaic disease of tobacco will be considered subsequently.

Nitzescu in 1914 (256), attributing pellagra to a maize intoxication, claimed that the Abderhalden reaction was regularly positive between pellagra sera and zein, but negative between healthy sera and zein. Subsequent investigations have largely discounted this work, however, and Herzfeld in particular (124) has shown that zein is regularly broken down by many types of sera, healthy, gravid, psychotic, etc.

## IV. SEROLOGY OF THE PLANT TOXALBUMINS AND OF NON-TOXIC SUBSTANCES AFFECTING BLOOD

An extensive number of toxic plant substances have been detected, purified to a greater or less extent, and studied from their serological aspects. The list includes the following:

Abrin (*Abrus precatorius*), agrostemmin (*Agrostemma Githago*), two or more substances found in *Amanita* species, condurangin (*Condurango blanco*), crepitin (*Hura crepitans*), crotin (*Croton tiglium*), cyclamin (*Cyclamen europaeum*), cytisin (*Cytisus* spp.), digitalin, digitonein, and digitonin (*Digitalis purpurea*), *Herniaria* sapotoxin, papain (*Carica papaya*), phasin (*Phaseolus*, *Vicia*, *Pisum*, *Ervum*, etc.), *Quillaja* sapotoxin (*Quillaja saponaria*), ricin (*Ricinus communis*), robin (*Robinia Pseudacacia*), *Sapindus* sapotoxin, saponin (*Saponaria officinalis*), senegin (*Polygala senega*), smilacin (*Smilax* spp.), solanidin and solanin (*Solanum* spp.), and *Yucca* saponin. Toxicodendrin (*Rhus toxicodendron* and other spp.) and the toxin of the "tutu" (*Coriaria thymifolia* and *C. ruscifolia*), both evidently glucosides, and the toxic alkaloids are considered in the following section.

Although the substances enumerated above are generally referred to as "toxalbumins," in many cases a critical demonstration of their albuminous or even of their protein nature is lacking. However, the term "toxalbumin" is retained in the present discussion, for the sake of convenient grouping and in uniformity with the usage of the earlier literature. Much of the work in this field originated in the laboratory of Kobert, and an extensive paper by him in 1913 (150) affords a comprehensive review of this work.

The plant toxins enumerated above very frequently show characteristic effects on normal blood. The normal washed blood corpuscles of various species are hemolyzed, often at very high titers, by agrostemmin (148), a constituent of *Amanita* (148, 85, 86, 1, 88, 91, 94, 284, 97), crotin (79, 135, 377), cyclamin (350, 148), digitonein and digitonin (148), *Quillaja* sapotoxin (269, 350, 148), ricin (148, 73), *Sapindus* sapotoxin (148), saponin (269, 149), senegin (10, 148), smilacin (148), solanidin (271), solanin (271, 273, 18, 148, 149, 20), and *Yucca* saponin (148), while papain has a lytic effect on serum proteins (65, 66). Hemolysis also occurs with such toxic alkaloids as cocain, atropin, strychnin, and pilocarpin, but not with a number of non-toxic alkaloids. Condurangin and cytisin lack such hemolytic power. Instead of hemolyzing normal blood corpuscles, or often as a

preliminary to hemolysis, the plant toxalbumins very frequently have the power of agglutinating washed corpuscles or of precipitating normal serum, or both. This is true of abrin (57, 287, 147, 122, 72, 79, 173, 149, 135, 171, 377), constituents of *Amanita* and other fungi (88, 91, 93, 94, 95, 284, 97), crotin (79, 173, 149, 171, 377, 57), phasin (170, 287, 375, 28), *Quillaja* sapotoxin (269), ricin (20, 223, 173, 287, 336, 72, 73, 79, 62, 173, 149, 158, 135, 9, 77, 171, 134, 227, 289, 377, 28, 57), robin (149, 173), and crepitin (293, 272), but not of condurangin (141).

The immunization of animals with the plant toxalbumins by feeding or injection of sublethal doses, is customarily followed by a greater or less resistance to or toleration of the poison in normally lethal amounts.

This has been shown in particular for abrin (72, 135, 57), *Amanita* (86, 92, 93, 284), crotin (57), papain (277), ricin (72, 135, 134, 226, 243, 57), robin (72), toxicodendrin (272), and crepitin (293, 272). Such acquired immunity is usually accompanied by the appearance in the serum of neutralizing antibodies, e.g. in the cases of abrin (72, 135, 57), *Amanita* toxin (85, 86, 284), crotin (57), ricin (72, 73, 183, 135, 134, 127, 57), toxicodendrin (272), and crepitin (272, 293). Solanin is claimed by Pohl (273, 274) to induce antihemolysis in the rabbit, but this has been contested by Bashford (18, 19). The ricin antitoxin may have a relatively simple structure since it is resistant to heating for 2 hours at 60°C. or for 1 hour with peptic acid at 35°C. (but not at 60°C.), and is not destroyed by incubating 1/2 hour at 37° in the presence of M/10 $H_2SO_4$ or M/10 NaOH (134).

With the immunization of animals to the plant toxalbumins and the production of resistance and neutralizing antibodies in the serum, there also frequently appear various other types of serological reactivity.

Thus acquired precipitins for the toxic material have been demonstrated in serum immune to crotin (20), papain (277), and ricin (183, 223, 226, 243, 28, 127, 314, 315), complement fixing antibodies in sera immune to papain (277) and ricin (377), and anaphylactins in the case of papain (276). Furthermore, such immune sera often specifically prevent the hemolysis or hemagglutination of normal bloods by the toxalbumins, as is true for antihemagglutination with abrin (287), *Amanita* (93, 97), phasin (287), and ricin (134, 135, 227; contested in 183), and for anti-hemolysis with *Amanita* hemolysin (86, 93), crotin (135), ricin (134), and solanin (272). The hemagglutinating and hemolytic actions of the phytotoxalbumins are demonstrated with washed red corpuscles. If whole or defibrinated blood is used, the serum present not infrequently prevents hemolysis or hemagglutination, even though the animal has not been previously immunized (20, 223, 134, 227, 173, 97). Peptone also has such an inhibitory effect (287).

The plant hemagglutinins are not necessarily the same as the hemolysins, the normal serum precipitants, or the toxins present in the same extracts (77, 94, 377); in fact each of these manifestations may be caused by a different factor in the plant extract, although in the case of ricin the amount of ricin neutralized by immune serum is equivalent to the amount which is precipitated by the same serum, which suggests that the two reactions may be homologous. Yet many extracts which agglutinate do not cause serum precipitation or hemolysis or are non-toxic, toxic extracts may be non-agglutinative, hemolyzing extracts may not cause agglutination, etc. The normal agglutination cannot be due merely to a difference in isoelectric points of the active substances, because of the peculiar elective characteristic of various agglutinins toward the same and different bloods. Neuberg (253) held that hemagglutination by plant extracts is associated with lipolysis, basing this contention on his and Rosenberg's observations on the lipolytic activity of commercial ricin and crotin, but Mendel (208) had questioned this view since Osborne's highly purified ricin showed high agglutinative power with little or no lipolysis, while certain highly lipolytic plant extracts were non-agglutinative.

The evidence is strong that the essential elements of most of the plant toxins considered here are protein.

Thus the blood effects of abrin are destroyed by heating at 85° (122), crotin at 69–70° (79), phallin at 65° (85, 86, 88, 94), papain at 100° (285), ricin at 90–100° (226, 227) or less (377, 264), and phasin at 90° (375). Similarly, these substances do not readily pass through dialyzing membranes, e.g. ricin (72), crotin (79), phasin (170), and *Amanita* toxin (148), and they are customarily precipitated by ammonium sulfate, alcohol, and other protein precipitants (crotin, 79; *Amanita* hemolysin, 86, 88; phasin, 170). Crotin has been shown to be a mixture of a globulin and an albumin and is destroyed by alkali and acid (79). Ricin appears to be a coagulable albumin, digestible by trypsin (264). The hemolysin of tomato which is active at dilutions of expressed sap up to 1:30 (50) does not pass through membranes which are permeable to the larger non-protein molecules but impermeable to protein. It is inactive at pH 4.7 but becomes active above pH 4.9 and below pH 4.5, and is hence probably a protein with isoelectric point near pH 4.7.

In spite of these ordinary properties of proteins, however, a number of workers have felt that there was some doubt as to the protein nature of the toxalbumins because they are often highly resistant to the action of proteolytic enzymes, as has been shown for ricin (241, but digested by trypsin according to 264), robin (173), abrin (173), *Amanita* toxin (85), and phasin (375). But, although the bacterial toxins are usually destroyed by such enzymes as pepsin and trypsin, many plant proteins are highly resistant to their action, and such a resistance is not incompatible with the view that the plant toxins considered here are protein in nature (272).

The evidence points to the view that the toxic principle of a plant toxin may be entirely distinct from the principle or principles affecting normal blood. Müller (241) and Jacoby (134) have both shown that the hemagglutinin of ricin is destroyed by pepsin, while the toxin is unaffected by this enzyme. Furthermore, in the case of *Amanita* the agglutinin is thermostable and non-antigenic, the lysin thermolabile and antigenic (94, 284), and anti-hemolytic sera do not protect against the toxin (93). The case of *Amanita* deserves special mention. Ford maintains that there are present in *Amanita* two substances, one, the *Amanita hemolysin* of Ford or *phallin* of Kobert which is a N- and S-containing glucoside, alcohol-precipitable, thermolabile (65°), and antigenic, leading to the production of anti-hemolytic sera, and a second, the *Amanita toxin* which is an indol or pyrrol derivative or an aromatic phenol so combined with an amine group that it gives an indol or pyrrol ring on fusion. It is thermostable and antigenic (94, 85, 1, 88, 89, 93). Kobert believes the *Amanita* toxin of Ford to be a non-antigenic alkaloid, the lysin a protein, not a glucoside (272). The anti-hemolytic sera do not protect against the toxin, although animals may be immunized against the toxin by using whole *Amanita* extracts as immunization antigens. Because the lysin is thermolabile, alcohol precipitable, and antigenic, Kobert (284) maintains that it must be proteinaceous, while he relegates the toxin to the class of toxic alkaloids. This *Amanita* toxin-lysin-agglutinin complex has never been satisfactorily unravelled.

The acquired serological reactions to the phytotoxalbumins show a well-defined specificity. Thus anti-ricin sera do not affect abrin, and reciprocally (72, 73), the two *Amanita* constituents are serologically distinct (85) in the sense that anti-hemolytic sera are not antitoxic, and the bean agglutinin is serologically different from those of ricin and abrin although there is cross-reactivity between the bean and lentil hemagglutinins (287). The complement fixation reaction with ricin sera is positive for ricin but negative for abrin and crotin (28). On the other hand, the plant toxalbumins do not have a high degree of elective capacity as regards the species of animal supplying

the agglutinated or hemolyzed blood, although all bloods are not affected equally, and many are unaffected by toxins agglutinating or hemolyzing other bloods.

The plant toxalbumins appear to illustrate the fact that the *in vivo* and *in vitro* effects of an antigen may be different. In the case of the *Amanita* toxin, to be sure, hemolysis takes place *in vivo* as well as *in vitro*, since injected animals are characterized by the appearance of red urine and hemolyzed serum (148). Crotin, however, shows that the acquired precipitin reaction against an antigen may be very different *in vivo* and *in vitro* (20). Thus if an animal is immunized to crotin its serum will give a highly active precipitin reaction with crotin. If the *precipitate* resulting is washed and injected into the circulatory system of an animal, death follows almost at once due to embolism. If on the other hand the crotin-immune animal is injected with *crotin*, embolism does not occur. If the *in vitro* effect were duplicated *in vivo*, the occurrence of the precipitate would cause death by embolism, and hence one may conclude that the two effects are different. The same difference has also been shown for peptone (20).

While the plant substances considered above are characterized by their toxicity, their blood effects, and their protein nature, there are also certain other, non-toxic protein substances with similar blood effects. Thus hemagglutinins for various species of blood have been found in *Salpiglossis* (76), *Datura* (9, 76, 77), *Phaseolus* (76, 28, 170, 287, 77, 208, 317, 155), *Pisum* (76, 170, 9, 208, 155), potato (195), *Ervum* (76), *Vicia* (76, 208, 155), *Lathyrus* (208, 9), *Soya* (9), *Canavalia* (9), *Glycine* (155), *Vigna* (155), and according to Mendel (208) also in the following: *Caragana, Cassia, Lens, Wistaria, Hesperis, Robinia.* They have been searched for but not found by Assmann (9), in henbane, by von Eisler and von Portheim (76) in species of *Solanum, Nicotiana, Capsicum, Physalis, Hyoscyamus*, and numerous other Solanacea, and by Mendel (208) in *Arachis, Baptisia, Cladrastis, Coronilla, Gleditsia, Gymnocladus, Lupinus, Mimosa* (?), *Psoralea, Thermopsis, Trifolium, Vigna, Brassica, Ipomea, Tropaeolum, Linum, Fagopyrum, Zea, Avena*, and *Canna.*

Besides the property of agglutinating blood corpuscles, non-toxic extracts of plants sometimes agglutinate bacteria or precipitate bacterial extracts, e.g. *Cotyledo* extracts (160, 161) and *Phaseolus* extracts (76), but not extracts of *Datura* (76) and agglutinating extracts of many other species of plants (208), or they may have the power of hemolyzing blood corpuscles, e.g. *Phaseolus* extracts (77), but not *Datura* extracts (76). Furthermore, as has been pointed out earlier in this paper, the extracts of a great many species of plants precipitate in the presence of normal blood serum when the extracts are at relatively high concentration.

It is evident that the non-toxic hemagglutinins of different plant species represent different substances, as is indicated by their elective capacity toward various bloods. Thus the extract of one plant species may agglutinate rabbit blood corpuscles but not human corpuscles, while the agglutinin of a second plant species may agglutinate human corpuscles while not affecting rabbit blood (208, 76). Furthermore the non-toxic plant agglutinins appear to be quite different from the agglutinins of the toxalbumins. Thus (76) anti-ricin serum inhibits ricin agglutination but does not affect *Datura* agglutination. It was not possible to prepare an anti-agglutinative serum for the *Datura* agglutinin (76), although this did succeed in the case of the bean (287, 155). Moreover some evidence has been adduced to show that the hemagglutinin and the hemoprecipitin of the bean are distinct entities since they have distinct thermal inactivation points (92° and 83°C. respectively, 317).

The plant agglutinins give evidence of being of the nature of resistant plant proteids. Thus they are destroyed in the

vicinity of 80°–90°C. of heat (77, 195, 155), are imperfectly dialyzable (161), are precipitable by ammonium sulfate and alcohol, are insoluble in fat solvents, and in the case of the bean at least are to be found only in the proteose fraction (317). On the other hand, as is often true of plant proteins, they are highly resistant to digestion by pepsin and trypsin (161, 317, 76). They are moderately resistant to ageing *in vitro* (161, 195). Activity titers range from the order of 1:20 to as high as 1:100,000 extract dilution (*Canavalia* extract, 9). The bean agglutinin appears to be associated with reserve food substances in the plant, since it is absent from the green seed, present in the ripe seed, and again disappears on about the 8th day after germination commences. It is absent from other tissues of the plant at all times (77, 217, 155). Normal serum has an inhibitory action on the agglutinin of the potato (195). It appears that the substance in the blood corpuscles responsible for the binding of the agglutinin is the same in different animal bloods with respect to the same plant agglutinin (195), and that the union of agglutinin and corpuscles may be dispersed by warming. This binding property resides in the stroma of the corpuscle, since plasmolyzed, washed stromata are still agglutinated, and it appears to be exceedingly resistant to chemical and physical treatments (288).

### V. SEROLOGY OF PLANT NON-PROTEINS

To speak of the serology of non-proteinaceous substances appears to be paradoxical since antigens are almost invariably proteinaceous (362, 363). Certain significant exceptions obtain to this rule, however, and furthermore a certain amount of evidence that the antigen *par excellence* is proteid rests on negative results with plant non-proteins. A word regarding serological tests of the plant non-proteins is hence not amiss.

*Carbohydrates.* From the work of Heidelberger and Avery it is known that a highly purified carbohydrate prepared from types II and III pneumococcus will precipitate at very high titers in the presence of sera prepared from the whole pneumococci, although in many cases such carbohydrates will not stimulate the production of antibodies.

Heidelberger, Avery, and Goebel (120) have observed that anti-pneumococcus sera also precipitate in the presence of various plant gums, and that purified gum arabic in particular shows this reaction at titers of 1:8,000,000 (anti-type II serum) and 1:2,000,000 (anti-type III serum). The absorption of the anti-pneumococcus sera with pneumococcus carbohydrate removes their activity against the gum arabic. Furthermore, Mueller and Tomcsik (240) have prepared from yeast a purified carbohydrate of yeast gum which precipitates in the presence of whole-yeast serum, but will not by itself stimulate the production of antibodies. Here the reaction occurs at a titer of 1:400,000. Similarly Tomcsik and Kurotchkin (348) were able to demonstrate anaphylaxis using animals sensitized passively with serum immune to whole yeast and shocking with the purified yeast-gum carbohydrate. The phenomenon in question has not been satisfactorily explained. Acting on the theory that certain lipoids (see below), while non-antigenic, will elicit antibodies if the lipoids are first linked to protein molecules, Mueller and Tomcsik attempted to link their yeast carbohydrate to horse serum protein and then immunize, but the attempts did not succeed. The carbohydrate technique was applied to species of *Monilia*, *Willia*, *Saccharomyces*, and *Trichophyton* by Kesten, Cook, Mott, and Jobling (145) who found that the agglutination and precipitin reactions thus obtained with these fungi showed only a low grade of cross specificity. When, however, an absorption technique was applied to this material, a high degree of specificity was observed among the forms studied. Similar results were obtained by Kurotchkin, Lim, and Chu (166, 167, 178) utilizing $H_2O$-soluble carbohydrate fractions in attempts to differentiate pathogenic species of *Trichophyton*, *Monilia*, and *Saccharomyces*. Injecting macerations of the whole fungi in immunisation, Kurotchkin and his associates obtained high-titer reactions with the carbohydrate fractions as test antigens (precipitation at 1:400,000, complement fixation at 1:25,000,000), but little

specific differentiation was observed among the various species of pathogens studied. At times heterologous reactions were even stronger than homologous ones. Wilcox and Link (376) found a common carbohydrate antigen in each of the 8 strains of *Neurospora sitophila* isolated from a single ascus, and in two inter-fertile, intra-sterile genotypes of *N. tetrasperma*.

It is difficult to interpret the specificity of these carbohydrate reactions. The low-grade specificity with the zoö-pathogenic yeast-like fungi may not be a function of the method but of the material, since numerous tests with protein fractions from such fungi also give a low order of specificity. The far-removed heterogenetic reactions between pneumococcus and yeast, coupled with the type differentiations in pneumococcus, indicate that the distribution of the water-soluble carbohydrate antigens has little bearing on the systematic relationships of the forms tested, representing an evolutionary specificity of an entirely different character from that of the proteins. The utility of the method has been shown in the differentiation of the strains of pneumococcus, and this may extend to strains or sexual types among the fungi. Wilcox and Link are continuing their studies in this direction. But from the standpoint of the use of serological reactions in plant systematics on a broader scale, the carbohydrate reactions, in the state of present knowledge, present such difficulties in interpretation as to limit their use to the narrowest differences in genetic constitution, a field where protein serology is as yet of little value.

Certain plant *glucosides* have also been studied serologically, and one in particular, a constituent of *Amanita*, has been discussed above. Similarly, the "tutu" plant (*Coriaria thymifolia* and *C. ruscifolia* of New Zealand) yields a crystalline glucoside which is highly toxic to livestock, thermostable, resistant to ageing and acids, and which shows no latent period in toxic action. Animals recovered from non-fatal toxic effects show no resistance to subsequent inoculation of lethal doses (92). It is not improbable that crepitin is also a glucoside (272, 293).

The toxicodendrin of the poison ivy (*Rhus toxicodendron*, *R. diversiloba*, and *R. venenata*) may be another glucoside of the same type (89, 90), although this is contested by Pick (272). Here a low order of immunity in man may at times exist naturally or may be obtained by enteral administration of the toxin, and rabbits, guinea pigs, goats, and horses if immunized with sublethal doses yield sera which neutralize 5–6 lethal doses of the toxin for guinea pigs (87, 89, 90). Straus (339a) has shown that the natural immunity in man depends upon an early exposure to the poison, as unexposed infants are regularly susceptible. Strickler's work in 1918 (340) indicated that *Rhus* poisoning may be effectively cured by the intramuscular injection of alcohol extracts of *Rhus*, and since then numerous cases of such effective treatment have been reported (e.g. Alderson, 2, Sayer, 310, Strickler, 341, 342, Bivings, 32, Williams and MacGregor, 381). Recently the less irritating almond oil has been substituted for alcohol as a vehicle for the toxin. Theimmunity resulting from such treatment is apparently a tissue immunity, transient in character (340). Serological tests as indices of immunity have proven unsuccessful (340, 342).

According to Porges (275), active unheated normal serum stimulates the phagocytosis of *starch* by homologous phagocytes, but this property of normal serum is not increased by immunization. Nozu (in 103a) claimed to have demonstrated an acquired complement deviation

reaction in rabbits inoculated with starch, but Giovanardi (103a) was unable to confirm this.

The toxic plant *alkaloids*, as strychnin, atropin, and pilocarpin, are hemolytic to normal blood, although this property was lacking in the non-toxic alkaloids tested (272). On the other hand, however, such alkaloids have not been found to induce in animals the production of antibodies or to react serologically with sera prepared from whole extracts of the plants yielding the alkaloids. Thus Lusini (184) obtained a precipitating serum for whole opium extract, but this serum did not react with the opium alkaloids morphin, narcein, codein, and papaverin. Similarly, a precipitating serum for whole belladonna extract failed to react with the belladonna alkaloids, and with morphin chlorhydrate and neutral atropin sulfate no precipitating sera could be prepared (186). Nicoletti (255) attempted to bind morphin to serum protein and produce sera specific for the alkaloid, but his attempts were unsuccessful, as were those in which he tried to produce morphin-precipitating sera by inoculation of morphin chlorhydrate.

*Chlorophyll* and *xanthophyll* extracts are hemolytic, the reaction of chlorophyll but not that of xanthophyll requiring sunlight, while normal serum of various species inhibits this action (246). It has been claimed by Hôdyô (128) that rabbits may be immunized with alcoholic chlorophyll extracts intracutaneously, and that the sera of such animals precipitate chlorophyll extracts with titers of 1:2000, while normal serum fails to give any reaction with chlorophyll. Although the related hemoglobin is known to be antigenic, these isolated findings of Hôdyô require further confirmation. If chlorophyll and xanthophyll are precipitinogenic in general, it is very surprising that one does not obtain common reactive factors in all green plants, a situation which assuredly does not obtain. Sherwood (326) has described a peculiar effect of normal serum in the presence of plantain extracts in liberating chlorophyll from chloroplasts, but the phenomenon is not at all understood. Elmore (80, 308b) has observed the liberation of chlorophyll from plant cells by the action of normal serum, a phenomenon which is not increased by immunization and which appears to be lipoid in nature.

*Hematoxylin*, a dye prepared from the wood of *Hematoxylon*, was claimed by de Angelis (5) to produce sera which precipitated and decolorized hematoxylin solutions, but Takemura (345) was unable to confirm this.

It is known from the work of Sachs and others (303) that while *lipoids* are unable by themselves to produce antibodies in animals, if the lipoid be first bound to a non-specific protein and then injected, the serum resulting is serologically active against the pure lipoid, a situation perhaps comparable to the action of the pneumococcus carbohydrate as detailed above. In such cases the lipoids are referred to as haptenes or half-antigens. Little has been done with the plant lipoids in this connection, however. Londini (182) was unable to immunize animals with either pure olive oil or pure cottonseed oil. Uhlenhuth and Händel (351), however, by immunizing guinea pigs with flax-seed oil later were able to obtain anaphylactic reactions by administering a second dose, not of the oil alone, but of an extract of the whole flax-seed. They also obtained similar results with rapeseed oil and almond oil. It is unlikely that the oil itself was the sensitizing substance. Almost incredibly small amounts of protein will sensitize a guinea pig, and it would be extremely difficult to

prepare a plant oil entirely free from such slight contaminations. The second or shock dose must be relatively larger, however, which might well explain why anaphylaxis was obtained by Uhlenhuth and Händel only when the second dose was of whole seed extract, not of the "pure" oil. Uhlenhuth and Jung (352) have described a reaction resembling agglutination of suspensions of olive oil and gum arabic in the presence of normal horse serum. Normal rabbit serum did not show such an action, but the inoculation of rabbits with the oil resulted in the production of sera which caused a similar flocculation. The "agglutinin" obtained was destroyed at 67° and was not specific for the olive oil, as other oils could be substituted in the reaction. It is possible that this phenomenon is interpretable in terms of the haptene theory.

*Organic acids.* Tannins have often been recognized as productive of non-specific artefact reactions with blood sera. The tannins in trade are frequently dispensed in the form of tannin-albuminates, but Schenk (311) has shown that, while such substances serve for immunization, a resultant reaction occurs only with the albumin, indicating that the tannin radical plays no part serologically in the reactions observed.

*(To be concluded)*

# MORPHOGENESIS OF THE SHOULDER ARCHITECTURE
## PART V. MONOTREMATA

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IN THE present series of papers concerning the shoulder architecture in vertebrates the Monotremata naturally follow the Reptilia. The former cannot be regarded as in any way representing the "prototherian" conditions in this feature of their anatomy, however, as one of their group names would imply. Rather must their shoulder be considered as a development of the reptilian type, highly specialized, and culminating, as well as terminating, in this group.

For more than 50 years the work of Westling on the anatomy of *Echidna*, and for 40, that of McKay on *Ornithorhynchus*, have been standard, and have been quoted far and wide. Both are excellent papers, indicating unusual care in dissecting, and the latter is further useful in that the author quotes extensively from previous reports on the subject. Westling's figures are excellent, but insufficiently diversified, while those of McKay are rather poor. If one desires purely topographical detail these papers are entirely adequate, but they are too old to reflect modern concepts of morphology, and present-day reports upon the anatomy of this group are highly desirable.

The shoulder region of a specimen of *Ornithorhynchus* was dissected, through the generosity of the U. S. National Museum, and the undertaking involved not a little difficulty. The muscles were particularly robust and closely crowded, the connective tissue about the nerves aggravatingly tough, and the smaller arteries so difficult to distinguish from nerves, that I suspect that the last feature accounts for some of the questionable innervations reported in the literature. Hence, in the present study the innervations often were verified microscopically.

### SKELETON

One is struck by the fact that in monotremes the shoulder girdle is definitely reptilian rather than mammalian in character. Not only is independent movement of the two sides so circumscribed as to be practically nonexistent, but the whole girdle is incapable of more movement than the sternum (and consequently the ribs) permits.

The membranous girdle is represented by clavicle and interclavicle only. The latter is broad posteriorly, where it articulates with the episternal element, moderately constricted in the middle, and expanded into a long, slender process on either side anteriorly. The latter extend to, and articulate with, the acromial processes of the scapula. Fused in the adult with the lateral interclavicular processes are clavicles, the whole being termed the clavo-interclavicular bars. To this bar are attached trapezius and clavicular deltoid elements as expected. In addition the superficial coracohumeral

has this origin, having relinquished original attachment to the anterior coracoid, as in placentals. The remainder of the interclavicle provides origin for a part of the pectoralis.

The dorsal division of the cartilaginous girdle may be viewed as consisting of suprascapula and scapula. The former, as a discernible element, has been reduced almost to the point of absence, except at one spot near the middle of the vertebral border, where there is a distinct cartilaginous process giving rise to a part of the levator scapulae dorsalis. The remainder of the suprazonal muscle matrix, consisting of serrati scapulae, rhomboideus, and levator scapulae (pars dorsalis in part, and pars ventralis in whole), has thus settled upon the scapula. As in reptiles mm. trapezius and teres minor (dorsalis scapulae), originally concerned with the cleithrum, also are attached to the scapula, while the scapular deltoid has migrated from the clavicle to this situation.

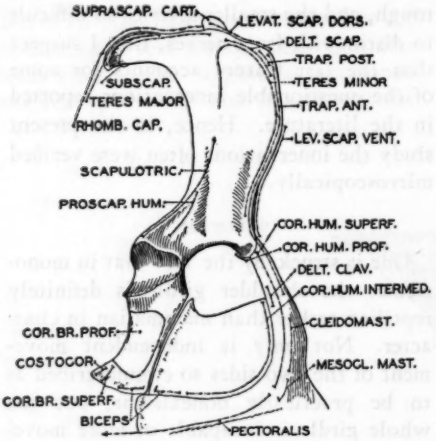


FIG. 1. DIAGRAMMATIC REPRESENTATION, IN FLATTENED, LATERAL PERSPECTIVE, OF THE RIGHT HALF OF THE SHOULDER GIRDLE OF THE PLATYPUS, WITH MUSCLE ATTACHMENTS SHOWN BY BROKEN LINES

As compared with the scapula of *Iguana*, that of the platypus is noteworthy in having the vertebral part prolonged caudally (instead of cranially) in the styliform process, the cranial border everted laterally, a process upon the medial surface, greater definition of the acromion and its separation from the coracoid. The reason for the caudal process is muscular, and it is difficult to see, in view of the immobility of the girdle, how the serrati could have been instrumental in this. It therefore must be due to mm. teres major and subscapularis, both of which elevate the caudal border of the humerus and thus rotate this segment.

For 85 years it has been believed (Owen, Mivart, Wilson and McKay, and others) that the anterior border of the scapula in monotremes, as in reptiles, represents the scapular spine of therian mammals, and that the part of the monotreme girdle representing the anterior border of the scapula in therians is the process upon the medial surface of the bone upon which is attached the omohyoid. The part between this process and the anterior border in platypus thus would represent the supraspinous fossa of therians, an area invaded by m. supraspinatus. The eversion of this representative of spina scapulae is quite marked. There is no violence done to sound morphology by this reasoning, for an exactly similar procedure has been followed within the class Mammalia by the development of the human crista iliaca.

Carrying this line of reasoning farther, Wilson and McKay advanced the hypothesis that the posterior border of the therian scapula is represented in monotremes by a ridge upon the lateral scapula between mm. teres minor (McKay's infraspinatus) and subscapularis. This is possible, but a needless complication for which there is no good evidence.

The question of the coracoids of monotremes needs no lengthy discussion here, for the situation is exactly the same as in reptiles. In primitive reptiles, as in lizards, an anterior and a posterior coracoid area give rise to distinctive muscle matrices, and the same muscles arise respectively from separate anterior and posterior coracoids in monotremes. For the former bone the term epicoracoid is permissible, but I prefer for the present to designate it by the English appellation.

In monotremes the anterior coracoid has made considerable progress in retreating from the scapula, an extensive hiatus separating the two, and articulation is solely with the posterior coracoid. It has a cartilaginous medial border that overlaps its antimere. Reduction in the extent of this bone evidently was permitted by the fact that a part of the coracohumeral matrix already has left it in favor of attachment to the clavointerclavicular bar, and a lesser part to the acromion. Of its original muscular equipment it retains only the intermediate part of the coracohumeralis, but to it has migrated the origin of anterior biceps and the extension, upon its dorsal surface, of the coracobrachialis profundus, both of which originally were concerned with the posterior coracoid. The latter muscle particularly may well have terminated a reduction of the bone that otherwise would be more pronounced. The character of this muscle, however, is doubtless a monotreme specialization, for it would be useless, in such form, in a mammal in which the arm is held in the typical therian position.

The posterior coracoid is the heavier but less extensive of the two ventral bones, and is not separated in adults from the scapula by suture. It provides attachment for mm. costocoracoideus, coracobrachiales, and biceps posterior. Noteworthy are the facts that it is relatively much longer in the platypus than in *Iguana*, giving a greater leverage to the muscles concerned, and that it articulates so definitely with the episternum.

Examination of the glenoid cavity of *Iguana* indicates that the humerus is, in mean position, held at an angle of about 30 degrees with the body axis, and essentially parallel with the ground. The comparable figure for *Ornithorhynchus* is about 70 degrees, with the humeral axis likewise horizontal. Indeed, the coracoid border of the glenoid projects to such a degree that the humerus cannot be lowered. In regard to brachial posture, then, the platypus is less "mammalian" than *Iguana* even, and less capable of bringing the arm beneath the body.

In certain respects the humerus of *Ornithorhynchus* is very remarkable. As it is held horizontally—a position which is very wasteful of effort when the animal is on land—the angles of leverage for the muscles must be great. This phenomenon is exhibited in the breadth of bone, arranged essentially in a tetrahedral pattern, reminiscent of *Eryops* and some of the earlier reptiles, but very different from living reptiles. Particularly noteworthy is the shape of the humeral head and its significance. The shape of the articular surface is not oval, as in *Iguana*, nor spherical as in so many mammals, but resembles an interrogation mark (Fig. 2A). The way in which this operates is at once clear if one manipulates the head within the glenoid cavity. It is then seen, with the humerus horizontal, that when the anterior part of the articular surface is within the glenoid, the manus is advanced and slightly abducted. As one rolls the articular surface through the glenoid the humerus is rotated, the distal end slightly retarded, and the manus

brought downward, backward, and finally against the body with the palm upward. This is the stroke movement which the animal uses in swimming, and by which it may walk. Elevation and depression of the distal humerus is possible to a limited extent but only within a very restricted arc.

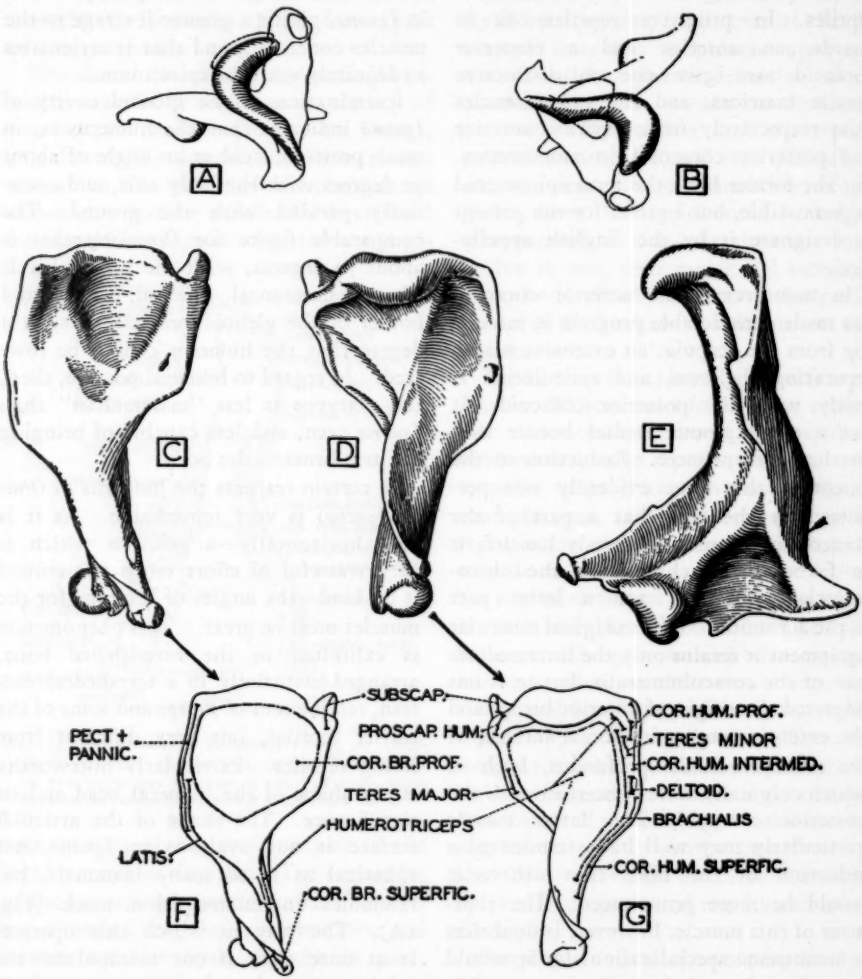


FIG. 2. DETAILS OF THE RIGHT HUMERUS OF THE PLATYPUS

*A* and *B*, proximal end of the right humerus of the platypus, the articular surface shaded and the position of the glenoid shown by a dotted line; to show degree of rotation possible: *A*, with anterior border of the humerus elevated, and *B*, with same depressed. *C*, the right humerus from ventral; *D*, from dorsal; and *E*, from cranial aspects. In the last the arrow pierces the entepicondylar foramen. *F*, ventral aspect of the humerus showing muscle attachments. *G*, the same from the dorsal aspect.

The proximal humerus is very broad (51 per cent of the length) and thin. The cranioventral margin may be termed the cranial border, and the caudodorsal margin the caudal border. Both represent processes lengthened to form crests. The dorsal surface of the proximal expansion is occupied by the humerotriceps origin, and the ventral surface by the insertion of the coracobrachialis profundus. The latissimus has shifted distally, as also has the coracobrachialis superficialis insertion.

Because the main action of the humerus is that of rotation the muscle attachments (except of humerotriceps and coracobrachialis superficialis) exhibit a tendency to congregate upon the bordering crests.

The plane of the distal part of the humerus is at a right angle to that of the proximal part, and is even more expanded, its width being 78 per cent of the humeral length. Most of this width is attributable to the medial epicondyle, which gives great strength to the forearm flexors, while the lateral epicondyle is shorter. The trochlea is offset considerably lateral to the axis of the bone and is spherical, allowing much rotation (supination and pronation) of the forearm as a whole. An entepicondylar foramen is present.

Recent morphologists mainly agree in the opinion that monotremes constitute one mammalian stock, while marsupials and placentals represent another; but seldom or never does one encounter in the literature an opinion regarding the sort of reptile from which monotremes may have arisen. Both the myology and osteology of this group exhibit a high degree of specialization, in many respects away from the direction taken by therian mammals. It is always a question in such cases which characters may be the result of adaptation and which may be basic; but there is one point, covered by the present paper, of particular interest in this regard. The theory of irreversibility of evolution is accepted by me only with extensive reservations, but it is difficult to see how the anterior coracoid of a tetrapod could become severely reduced, following the shifting of its musculature to other bones, as was clearly the case with the theriodonts of the Triassic, and could later take the reverse step to conform to monotreme conditions. The obvious conclusion is that monotremes could not have been derived from any of the "mammal-like reptiles" of therapsid affinity of the types now known.

### NERVES

The brachial plexus of *Ornithorhynchus* is built upon the usual principle comprising three trunks, derived mainly from five roots. The first trunk is formed by $C_5$ and C6, the second by $C_7$, and the third by C8 and $T_1$; hence the axis is through $C_7$. In addition the plexus may receive a fine filament from $C_4$, but at least in some cases it is not unlikely that this carries solely phrenic fibers. Westling and McKay found that $T_2$ also may contribute to the plexus, but I was unable to demonstrate this in my specimen.

The nerves of the plexus are even more readily separable into dorsal and ventral groups than in most mammals. The definition of the usual mammalian condition, involving three cords, is obscured; or if one prefer it can be stated that there is a cranial dorsal, a middle ventral, and a caudal mixed cord.

The brachial plexuses of all tetrapods have many points that are fundamentally very similar, but no one who has completely dissected the appendage can consider that the plexus of the platypus is fundamentally mammalian (therian). It is different in a very significant manner, and its most unique feature, involving

the double radial nerve, is quite comparable to the situation encountered in both Amphibia and lacertilian reptiles. Neither do the trunks of C8 and T1 supply only ventral components as stated by Miller. Other interesting features comprise the facts that n. thoracodorsalis has relinquished close association with the axillary group and arises from the second rather than the first cord, and the arrangement of the cord chiefly supplying n. medianus.

The illustration of the plexus is shown from the dorsal aspect, contrary to the

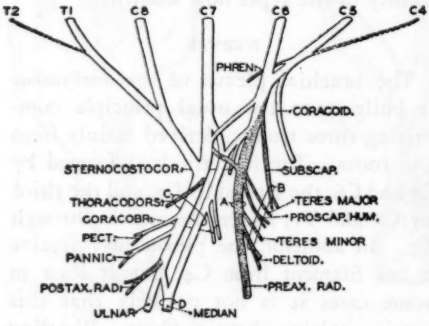


FIG. 3. RIGHT BRACHIAL PLEXUS OF THE PLATYPUS FROM DORSAL ASPECT

*A*, passes deep to scapulotriceps; *B*, axillary group; *C*, passes caudal to latissimus; and *D*, passes through entepicondylar foramen.

usual practice, for the reason that this is the necessary approach for its dissection.

*N. vago-accessorius* supplies mm. trapezius, a part of the cutaneous muscle field, and the two divisions of "sternocleidomastoideus." At least C2, 3, and possibly also 4, enter this field.

*Nn. infrahyoidei*, innervating the infrahyoid musculature and representing nn. hypobranchiales of amphibians and reptiles, are derived from C1 and 2.

### Dorsal (extensor) division

#### Suprazonal group

The nerves representing levator scapulae branches and nn. thoracales posteriores of placentals (not illustrated), arise from C3 to 7, and innervate mm. serrati anteriores, rhomboideus, and levatores scapulae.

#### Shoulder group

*N. thoracodorsalis*, supplying m. latissimus dorsi, is derived at least from C7 and possibly more posterior roots. In the platypus it has relinquished close association with the axillary group.

*Nn. axillares communes* consist of the remaining dorsal divisions of the shoulder proper, and in the platypus they diverge from the trunk of the preaxial radial, the highest component being one or more branches to m. subscapularis, and next, n. teres major. The former, and at times the latter, are so situated as to receive no fibers from nerve roots caudal to C6, while C7 also can contribute to the remainder of the axillary group. The main trunk then passes caudal to m. teres major and thence cranially deep to the origin of m. scapulotriceps. In consecutive order branches are supplied to mm. proscapulohumeralis, teres minor, and both divisions of deltoideus.

#### Brachio-antibrachial group

*Nn. radiales*. The dorsal fibers to the extremity proper follow two pathways in an interesting way. The first (a) comprises a continuation of the cranial cord from which arise the axillary branches, and hence may be considered as preaxial in that it is derived from C5, 6, and possibly partially from 7. After passing in a cranial direction deep to the original end of the scapulotriceps it follows a superficial course, lateral to m. humerotriceps, to the forearm, where it divides into rami profundi (muscular) et cutanei. The second part (b), which may be designated as postaxial, separates near the elbow from the third or caudal cord of the

plexus (whose course is described under the ventral nerves), enters between scapulo- and humerotriceps and is distributed to these muscles. This arrangement accordingly is comparable to conditions in some Amphibia and in lacertilians, but not in therian mammals.

### Ventral (flexor) division

#### Infrazonal group

These arise either from $C_5$ or 6, or both, and take the form of fine filaments from the middle cord of the plexus, innervating mm. sterno- and costocoracoidei.

#### Shoulder group

*Nn. pectoralis et panniculus carnosus* are two stout branches of the caudal cord of the plexus and are distributed to the muscles of the same name. Upon one side of the animal dissected m. pectoralis appeared to receive a branch also from the middle cord, but it was cut in skinning and I could not be certain. This was not the case upon the other side.

*N. coracoideus* (anterior coracoid nerve) is derived from $C_5$ alone. It passes to the lateral aspect of the girdle through the fenestration bounded by scapula, coracoid, and clavicle, and is distributed to the three divisions of the coracohumeral musculature, and, by a fine filament, to a part of m. biceps anterior.

Nn. coracobrachiales in the specimen dissected occurred as more than one branch of the middle cord of the plexus. Mm. coracobrachiales, biceps posterior, and a portion of biceps anterior are so supplied.

*Nn. flexores antibrachii* (flexor brachii, brachialis longus inferior) comprise median and ulnar components. The former constitutes a direct continuation of the middle, and the latter of the caudal cord of the plexus. Both together pass caudal to the latissimus, between the latter and the coracobrachialis superficialis, and in the distal brachium lie superficial. After the posterior trunk gives off the postaxial branch of the radial it makes an anastomosing contribution to the median, and then continues around the elbow as n. ulnaris. After receiving the anastomosis above mentioned the continuation of the middle cord pierces the entepicondylar foramen and becomes n. medianus. It then innervates m. brachialis.

#### MUSCLES

McKay has given not only a full description of the myological details, but extensive digests of the opinions of previous workers in this field. Muscle descriptions in the present paper, therefore, are made as concise as feasible, while reference will be made to previous reports only when these appear to be called for.

### Branchiomeric division

*M. trapezius*. This occurs in two divergent divisions: one (a) arises from the dorsal fascia as far caudally as the latissimus origin, and narrows to insert upon the anterior angle of the scapula. The other (b) has fascial origin over almost the entire braincase and from the anterior part of the nuchal line. It converges to insert upon the anterior scapular border at the anterior angle and ventrally, upon the acromion and almost the entire clavicle. The anterior border of this muscle is quite thick. Berry Campbell (personal communication) found that the cutaneous muscle field has had increment from the accessory field, but my specimen was in such condition that the cutaneous muscles were not fully dissected.

*M. mesocleidomastoideus* (episternocleidomastoideus part McKay) arises under cover of the anterior trapezius from the squamosal. More ventrally it emerges from beneath the border of the trapezius

as a narrow band and inserts upon the interclavicle near the midline.

*M. cleidomastoideus* (episternocleidomastoideus part McKay) arises from the squamosal also, immediately posterior to the last division, than which it is narrower. It inserts upon the middle portion of the clavicle.

Innervation: N. accessorius, C2, 3, and probably 4. With particular care I demonstrated that C2 and 3 ramify into the anterior trapezius and extend to the mastoid divisions. C4 also enters the trapezius but I was less certain of its ramifications.

*Infrahyoid division*

There are two muscles in this group that may receive the names employed. They may or may not be exactly homologous with the corresponding two divisions of placentals.

*M. omohyoideus* arises from the process upon the medial side of the scapula, as described under the skeleton, and inserts upon the hyoid and mylohyoid raphe.

*M. sternohyoideus* arises from a midline raphe dorsal to the coracoids as far caudally as the sternum, and inserts upon the hyoid.

Innervation; Branches of C1 and 2, the former of which receives also a contribution from XII.

**Dorsal (extensor) division**

Suprazonal (or serratus) matrix

*M. rhomboideus* arises broadly from the cranium and slightly from the ligamentum nuchae, and inserts upon the caudal half of the vertebral border of the scapula.

*M. levator scapulae dorsalis* arises robustly from the cervical transverse processes (2–7, McKay), and inserts upon the anterior half of the vertebral border of the scapula, including the small suprascapular cartilage.

*M. levator scapulae ventralis* (acromiotrachelien McKay) arises from the cervical vertebrae and inserts upon the ventral two-thirds of the anterior surface of the reflected part of the craniomedial border of the scapula, including the acromion, encroaching slightly upon the clavicle.

*Mm. serrati scapulae.* The origin of these slips was not checked accurately, as this could not be done without interfering with more important features. There is a separate posterior slip (superficialis) (Fig. 4B) to the caudal half of the vertebral border, a largely horizontal division, unmentioned by McKay, the most lateral of all, from a costal origin to insertion upon the ridge upon the medial scapula, and the most medial division, comprising a sheet of slips from the ribs to the entire vertebral border of the scapula.

Innervation: C6 to 7, the last supplying the most caudal (superficialis) slip.

SHOULDER GROUP

Thoracodorsal (latissimus) matrix

*M. latissimus dorsi.* I found this to be somewhat different than described by McKay. Like his specimen mine showed a dorsal portion, arising from the vertebral spines, and a costal portion from the ribs. The former, however, is separable into three layers (not shown in Fig. 4) which partly overlap. The deepest is continuous with the costal portion. Many of the fibers of the most superficial layer disappear in fascia, and the others join a common insertion, upon the ridge extending from the entepicondylar foramen toward the extensor process of the proximal humerus. The insertion of the ventral part of the muscle is more tendinous. The whole constitutes a very powerful mechanism for retarding and then elevating the arm. This doubtless is an aquatic specialization. It should be noted that

the latissimus insertion, unlike conditions in reptiles, is not straddled by any part of the triceps but is located medial to the latter complex. On the other hand it is situated between the two divisions of the extensor nerve (n. radialis).

Innervation: N. thoracodorsalis, from $C_7$, 8, and probably T1.

*M. deltoideus clavicularis.* This is the deeper division of McKay's deltoideus acromioclavicularis. It arises from the clavointerclavicular bar deep to the coracohumeralis intermedius, in intimate association with the origin of the latter, and inserts just deep to the insertion of the scapular deltoid.

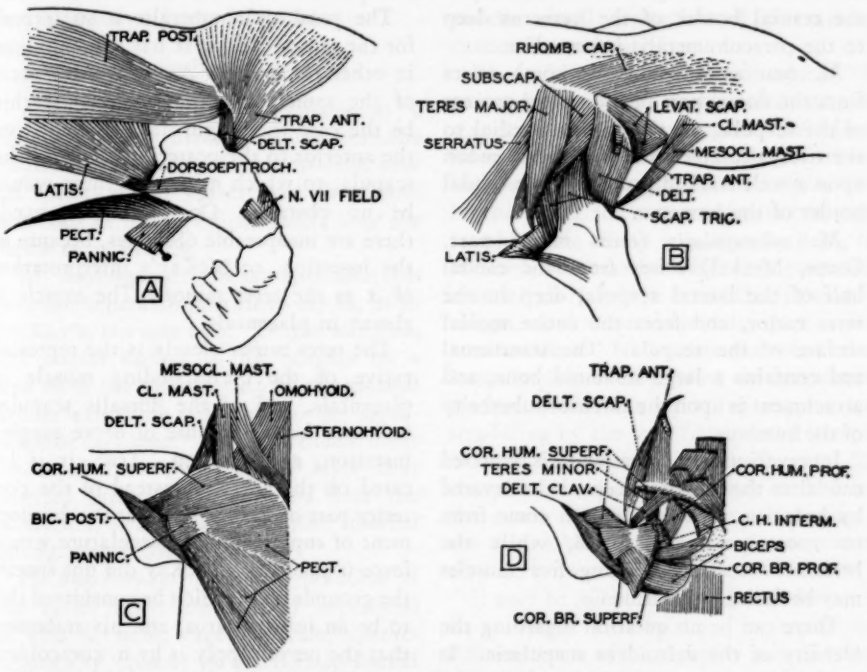


FIG. 4. RIGHT SHOULDER MUSCLES OF THE PLATYPUS

*A*, lateral view of superficial muscles; *B*, lateral view of deeper details; *C*, ventral view of superficial muscles; and *D*, ventral view of deeper details.

## Axillary matrix

*M. deltoideus scapularis* (all authors) arises from the anterior angle of the scapula adjacent to the insertion of the trapezius posterior. It passes between the brachial musculature and m. coracohumeralis superficialis to insert by a constricted tendon upon a well defined tubercle upon the cranial border of the humerus.

*M. proscapulohumeralis* (teres minor McKay; subscapularis part, Coues, Westling). This term, although to some extent a misnomer, is applied for the reason that the muscle appears to be the homologue of the shorter division of the same name in caudate amphibians. It is a short, thick, deep muscle arising from the ventral third of the glenoid border of the scapula, between the teres minor and scapular

triceps, anteriorly overlapping in part the origin of the latter muscle. Insertion is upon the tip of the caudal border of the humerus.

*M. teres minor* (infraspinatus McKay, Owen, Coues, Cuvier and Laurillard) arises deep to the deltoid from the anterior part of the lateral scapula and ventrally almost to the acromion. It inserts upon the cranial border of the humerus deep to the coracohumeralis intermedius.

*M. teres major* (most authors) arises from the dorsal part of the caudal process of the scapula. It passes caudomedial to the triceps complex and inserts by tendon upon a well marked process of the caudal border of the humerus.

*M. subscapularis* (teres major part, Coues, Meckel) arises from the caudal half of the lateral scapula, deep to the teres major, and from the entire medial surface of the scapula. The insertional end contains a large sesamoid bone, and attachment is upon the extensor tuberosity of the humerus.

Innervation: M. subscapularis situated caudal to the scapulotriceps, is innervated by branches whose fibers can come from no roots posterior to C6, while the branches to the remaining five muscles may be derived also from C7.

There can be no question regarding the identity of the deltoideus scapularis. It carries to an extreme the tendency for dorsal migration exhibited throughout its phylogeny, and here represents a spinodeltoid. McKay included the deltoideus clavicularis with the superficial coracohumeral, terming the two together deltoideus acromioclavicularis. This, in view of the distinctiveness of the innervation, is difficult to understand. That it is probably phylogenetically distinct from the coracohumeral is first suggested by the separation of the two insertions by the scapular deltoid, and that it belongs with the latter is shown by the common nerve supply. McKay may have been led astray about the innervation by the fact that branches of the n. coracoideus lie in contact with it. The muscle evidently represents the deeper layer of the deltoid (scapulohumeralis brevis of Caudata), over which the coracohumeralis has extended.

The proscapulohumeralis is so termed for the reason that if it has a homologue in other tetrapods it must be the muscle of the same name in Caudata. If this be the case the origin has changed from the anterior to the posterior border of the scapula, to which migration there would be no obstacle. On the other hand, there are insuperable obstacles, because of the insertion, to McKay's interpretation of it as the teres minor. The muscle is absent in placentals.

The teres minor clearly is the representative of the corresponding muscle in placentals, and of the dorsalis scapulae in lower forms, because of nerve supply, insertion, and position. True, it is located on the anterior instead of the posterior part of the scapula, but the development of suprascapular musculature would force it caudally. McKay did not specify the grounds upon which he considered this to be an infraspinatus, and his statement that the nerve supply is by n. coracoideus as well as n. axillaris is probably attributable to the fact that the muscle receives blood from a small artery accompanying n. coracoideus and this vessel easily is mistaken for a nerve.

M. teres major is even more distinctive a muscle than in placentals, and if it has been derived from the latissimus dorsi, as somewhat reluctantly I have come to believe, neither its conformation nor nerve supply here indicate the fact; yet there appears to be no other disposition to make of it. In mammals its insertion

is always in intimate relation with that of the latissimus, and there is no muscle in anamniotes that could correspond to it. Hence it is assumed that a part of the latissimus became attached to the glenovertebral angle of the scapula, and that this part then split from the remainder, as it very easily could do. The subscapularis is essentially the same as in reptiles, and calls for no further remarks.

### Brachio-antibrachial group

*M. dorso-epitrochlearis* originates as a slender but very distinct muscle from the ventrolateral border of the latissimus near its insertion. It passes over the elbow and ends in the superficial fascia over the flexor carpi ulnaris.

*M. scapulotriceps*. In my specimen this was not separable into three layers, as in McKay's, but into two. Origin was from the lateral surface of the scapula on a line from the ventral part of the glenoid border extending half way to the suprascapular cartilage. The more caudal fibers separated into a superficial layer that inserted into the loose fascia over the olecranon, while the deeper part inserted strongly upon the olecranon.

*M. humerotriceps*. As both branches of the radial nerve are entirely superficial to this division, caput laterale, characteristically present in placentals, should be considered, strictly speaking, as undeveloped in monotremes. In the latter there are three divisions, however; (a) a more lateral part, arising by a thin tendon from the cranial portion of the dorsal surface of the proximal humerus, (b) a main portion, from the entire dorsal expansion, and (c) a deep, short division from the caudal surface of the distal expansion of the humerus. All insert upon the olecranon.

Innervation: The nerve supply of m. dorso-epitrochlearis was not determined by me. Westling, and McKay, indicated that the nerve is given off n. radialis. The triceps is supplied by the postaxial radial.

As m. dorso-epitrochlearis is reputedly innervated by a branch of n. radialis it is placed with the triceps. This branch, however, very readily could have shifted from association with n. thoracodorsalis, so the actual derivation of the muscle cannot be established.

As in reptiles, the scapulotriceps of monotremes appears to have reached the scapula by migration along a fascial plane in order to bridge the axillary nerves. Because of the extent of the origin, robustness, and the extreme dorsal position in which the elbow is habitually held, it is clearly a specialized and very powerful muscle. It should be noted that in monotremes, as in other mammals, there is no corocotriceps. It is thought that the latter is present only when the latissimus tendon is so situated as to require its straddling by the long head of the triceps in order to render the latter efficient. In the platypus the latissimus is medial to the entire triceps complex; and besides, the arm is held in such a position that a coracotriceps could not function.

It may be noted that the humerotriceps is a closely knit entity. In tetrapods different parts of the complex may diverge proximally in various ways. Thus in *Iguana* a lateral and medial head straddle the insertions of proscapulohumeralis and latissimus. In *Ornithorhynchus* both these insertions are situated more caudally, so that the entire humerotriceps is located cranial to them.

## Ventral (flexor) division

### Infrazonal matrix

*M. sternocoracoideus* (sterno-epicoracoid McKay) is the more dorsal and narrower of these two muscles. It arises from the

dorsal aspect of the sternum and the medial part of rib I. It then becomes styliform, inserting upon the extreme craniomedial part of the posterior coracoid, and the medial cartilaginous border of the anterior coracoid.

*M. costocoracoideus* is fan-shaped, arising from more laterally on rib I, and converging to a restricted insertion upon the caudomedial part of the posterior coracoid.

Innervation: C(4), 5, and 6.

Owen, and Coues, with reservations, considered these muscles to represent m. pectoralis minor, but the former division usually is regarded as representing m. subclavius of placentals. The latter supposition is plausible, although it cannot be proven that the subclavius has not arisen from the anterior coracoid matrix.

SHOULDER GROUP

Pectoral matrix

*M. panniculus carnosus*. The older concept held that all dermal muscles posterior to the head constituted panniculus elements, but it appears that a considerable part of this muscle complex has been developed by posterior migration of the facial field, and hence, is innervated by cranial n. VII (see Huber, 1930). Unfortunately, the condition of the specimen at my disposal is such as to preclude the possibility of satisfactorily determining the innervation of each of the layers of such a complicated mass. The significant facts in the present connection are that the facial field has invaded the thoracic area and that slips of it insert not only upon the fascia of the trunk, but upon the antibrachium. Thus a more superficial part inserts into the fascia of the ulnar border of the wrist, and a deeper, more robust part, firmly upon the ulnar border of the distal ulna. These advance and elevate the manus. The panniculus itself, in several layers, is situated mostly ventrally and fuses at insertion with the pectoralis, the common attachment being to the flexor tuberosity of the humerus, with an inclination that retracts the arm.

*M. pectoralis* (pectoralis major part, all authors) arises from the midline ventral to the interclavicle and the entire sternal complex. The central fibers insert upon a raphe as illustrated. A small portion upon the posterior border is separable (McKay's pectoralis quartus), this being entirely comparable to the pectoralis abdominis of many placentals, but not of man. It inserts, with the panniculus, upon the entire crest of the cranial border of the humerus.

Innervation: Ventral branches derived from the more posterior part of the plexus and diverging from the caudal cord. Upon one side of the animal dissected the pectoral may have received a branch also from the middle cord. McKay stated that the panniculus is supplied as well by the lateral cutaneous nerve of the thorax, branches from C1–4, and from the posterior divisions of the spinal nerves. That these branches as listed by McKay are not cutaneous nerves piercing the panniculus requires proof.

Apparently tetrapods did not develop a panniculus until the pliable integument of the Mammalia was attained. There is every reason for believing, and none for doubting, that it was derived from the matrix of the primitive pectoralis. That the mammal lowest in the scale has one of the most complicated systems of dermal trunk musculature is of no concern to us here. There is nothing noteworthy about m. pectoralis except that its nerve supply exhibits a tendency to shift cranially—a phenomenon not in accord with the placental condition. Thus in some instances it receives in monotremes no increment from C8 to T1, as I found, although in other individuals of platypus it does so

(McKay). This matrix is certainly the representative of the pectoralis minor layer of higher mammals, this including mm. pectoralis minor, pectoralis abdominis, and panniculus carnosus where they occur. In this respect monotremes thus are intermediate between reptiles, in which the true pectoralis has over-ridden the coracohumeral, and placentals, in which the superficial coracohumeral (pectoralis major) has over-ridden the true pectoral layer.

Anterior coracoid matrix

*M. coracohumeralis superficialis* (deltoideus acromioclavicularis McKay, Westling; pectoralis major Meckel, Coues, Cuvier and Laurillard) is the superficial muscle adjoining the pectoral that arises from the entire clavo-interclavicular bar. It inserts upon a line on the anterior humerus extending from the deltoid process almost to the trochlea.

*M. coracohumeralis intermedius* (epicoracohumeralis McKay; deltoideus part, Owen, Coues; pectoralis major part, Cuvier and Laurillard; supracoracoideus part) lies immediately deep to the last, and is partially covered also by the pectoralis. It arises from the cranial two-thirds of the border of the anterior coracoid, and narrows to an insertion upon the cranial border of the proximal humerus.

*M. coracohumeralis profundus* (supraspinatus of authors) is in the same layer as, and adjacent to, the insertional end of the teres minor. It arises just medial to the ventral border of the acromion and inserts upon the more capsular part of the cranial border of the proximal humerus.

Innervation: N. coracoideus, from C5.

Meckel, Cuvier and Laurillard, and Coues correctly interpreted the superficial division of this matrix as representing m. pectoralis major of placentals, Westling indicated doubt, while McKay considered it to be a part of the deltoid. McKay gave the innervation as n. axillaris and possibly a minute twig from n. supracoracoideus (coracoideus). I found it very plainly innervated by the latter alone, placing its homology beyond question. In reptiles it lies deep to m. pectoralis, arising from the anterior coracoid, while in placentals it is entirely superficial to the true pectoral. In monotremes it is fairly intermediate in this respect, lying in the same layer with, and adjoining, the true pectoral. In this group, however, origin of the superficial portion has shifted from the anterior coracoid to the clavo-interclavicular bar, in this respect being suggestive of placentals.

The intermediate and deep layers of the anterior coracoid matrix may be discussed together. Owen, and Coues, considered the former to represent a part of the deltoid, and Cuvier and Laurillard, and Meckel, a part of the pectoral, while McKay advanced no opinion. Authors are pretty much in agreement, however, in considering the deepest division to represent m. supraspinatus.

It is extremely likely that in the broad sense these two muscles of monotremes represent mm. supraspinatus, infraspinatus, and possibly the anterior part of subscapularis of placentals, but it cannot be stated without reservation that the deepest division of the former sort of mammal represents any particular one of the three muscles in the latter sort. The deepest slip is excellently situated to invade either the anterior aspect of the scapula as a supraspinatus, to broaden and become both supra- and infraspinatus, or to be crowded caudally to become an infraspinatus only. On the other hand, the intermediate division might very easily migrate dorsally to become either one or both of the suprascapular

muscles. It either did this or was absent in the placental ancestor.

In this connection it must be remembered that the monotremes are very much specialized, and that in them suprascapular musculature, although definitely suggested, apparently has been arrested far short of the conditions indicated in Permian dicynodonts, or even some of the cotylosaurs. Accordingly the precise arrangement of the deeper coracoid musculature that finally gave rise to true suprascapular musculature may have been quite different from that now encountered in monotremes. All that can be said with conviction is that the superficial part of the matrix became m. pectoralis major, and the deeper part the suprascapular musculature, possibly including the anterior portion of the m. subscapularis. The last point will be discussed in conjunction with placentals.

### Posterior coracoid matrix and brachio-antibrachial group

As in placental mammals, but in a different fashion, these two groups of muscles in monotremes are closely associated, and they present as many difficulties as in lower tetrapods.

*M. coracobrachialis superficialis* (c. longus McKay) is located upon, and is partially hidden by, the caudal border of the posterior biceps. It arises from the caudomedial part of the posterior coracoid and inserts upon the medial epicondyle of the humerus.

*M. coracobrachialis profundus* (c. brevis plus epicoracobrachialis McKay). The more ventral part of this lies deep to the posterior biceps. Origin is from most of the ventral surface of the posterior coracoid, and in addition from the dorsal surface of both this bone and the anterior coracoid. Insertion is upon the entire dorsal surface of the proximal expansion of the humerus.

*M. biceps anterior* (caput epicoracoideus McKay) is a slender muscle caudal to m. coracohumeralis intermedius. It arises from the caudomedial part of the anterior coracoid and forms a slender tendon that partially fuses with the next.

*M. biceps posterior* (caput coracoideus McKay) caudally adjoins the last, just deep to the pectoralis. It arises from the medial part of the posterior coracoid and forms a rather broad tendon that inserts upon the radius.

Innervation: Nn. coracobrachiales, from the middle or flexor cord of the plexus, derived from C7 *et ante:* m. biceps anterior also by a filament from n. coracoideus.

*M. brachialis* is a stout muscle arising from most of the length of the humerus, between the humerotriceps on the one hand and the insertion of the deltoid and associated muscles on the other. It passes to the forearm with the biceps and is inserted upon the ulna.

Innervation: A branch of the median nerve that diverges distal to the entepicondylar foramen.

M. coracobrachialis superficialis calls for no comment. The deeper division is not only indivisible, in contrast to the situation in reptiles, but has spread around the posterior coracoid border and over the dorsal surface of both posterior and anterior coracoids in a quite remarkable manner, so that this dorsal part bears a considerable resemblance to m. subcoracoideus of reptiles, with which it frequently has been considered as homologous. The resemblance, however, is only superficial. In reptiles m. subcoracoideus is closely associated, both in conformation and nerve supply, with the dorsal subscapularis, and there is no reasonable doubt that it was derived from the latter. It is separated from the coracobrachial field by the strong posterior process of the coracoid. In the

platypus, on the contrary, the muscle occupying this situation not only is continuous, at origin and insertion, with the deep coracobrachial, but the nerve supply is the same, both very definitely by ventral components.

Any flexor muscle arising from the shoulder and inserting upon the forearm is here termed biceps. As explained in previous papers the earliest tetrapods evidently had no such muscle, but its need can be inferred from the fact that all living forms have endeavored to manufacture a biceps from such material as was at hand. The results are extremely diverse and different in Caudata, Salientia, Reptilia, and Monotremata. In reptiles conditions suggest that the distal part of the two divisions of the biceps have split from the brachialis component, while the proximal part of the anterior coracoid head has been derived from the coracobrachial matrix, as its innervation shows. In the platypus the biceps may have arisen in the same way, although it alone, without any reference to other tetrapods, shows no indication of it. If this were the case, then in some ancestor the entepicondylar foramen surrounded n. medianus at a point between the nerve branches to mm. brachialis and biceps respectively. The former thus was prevented from migrating in a proximal direction, while the latter could, and did, so migrate in conformity with any dictates of fasciculation.

As in reptiles the origins of the biceps could reach their present positions by migrating along fascial planes.

As in reptiles, also, one division of the biceps arises from the posterior coracoid, while the other has left its logical position by migrating to the anterior coracoid. Perhaps playing some part in the latter circumstance may be the fact that a slip from the anterior coracoid matrix has fused with the longer biceps, as a portion of the coracobrachial matrix has so fused in reptiles. In the platypus a nerve filament from branches supplying mm. coracobrachiales was found to enter the biceps anterior on one border, and a similar filament from n. coracoideus from the opposite direction. Histological examination showed that neither was a blood vessel, but that both were nerves.

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# A SPURIOUS PORTRAIT OF SWAMMERDAM

By G. H. PARKER

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IT DOES not seem to be generally known in America that a lithographic portrait designated as that of Jan Swammerdam and seen on the walls of many of our biological laboratories is spurious. This lithograph which is reproduced in Figure 1 of this article is marked "Jan Swammerdam geboren 12 February 1637 overleden 17 February 1680 te Amsterdam." Next the lower edge of the lithograph centrally placed and in small letters is the inscription "J. P. Berghaus lith Leyden 18 $\frac{22}{III}$ 51 en gedr."; at the right hand "J. Stolker del." and at the left "Rembrandt pinxt." On a sheet of paper projecting from under a book on the table within the picture itself occur the letters "Rembr," the inference being that the portrait was painted by Rembrandt. This portrait is reproduced in W. A. Locy's *Biology and Its Makers*, page 69 (1908) and in D. C. Peattie's *Green Laurels* opposite page 38 (1936), both books much too good to carry a misleading illustration. Locy refers to it in his text with the comment "although its authenticity has been questioned, it is the only known portrait of Swammerdam." Peattie apparently accepts it as genuine. It is against unwitting appropriations of this assumed representation of the celebrated Dutch micro-anatomist that this article is written.

To anyone familiar with the works of Rembrandt the background of the Swammerdam picture shows no sign of the master's style. If the portrait came from Rembrandt its delineator certainly took the greatest liberties with the possible original. But from several Dutch correspondents I have complete assurance that there never was a portrait of Swammerdam. Certainly the circumstances of his short, bitter, and impoverished life make it highly improbable that his face should ever have been put upon canvas. A careful survey of the illustrated catalogues of Rembrandt's works gave no record of such a painting, but in the course of this search the Keeper of Prints in the Fogg Art Museum, Miss Laura H. Dudley, called my attention to the resemblance between one of the heads in the Anatomy Lesson of Dr. Tulp by Rembrandt and the so-called head of Swammerdam. The suspected head in the Rembrandt painting is that nearest the head of Dr. Tulp himself (Figure 2). The resemblance of the figure in the assumed portrait of Swammerdam to the one just pointed out is striking (Figure 3). The neck dresses in the two are not the same, being a ruff in the Rembrandt head and a smoother neck circle in the lithograph, a form of neck dress more like that worn by the other auditors in the Lesson. The paper held in the left hand of Rembrandt's auditor has large lettering upon it which though illegible for the most

part gives no sign of having to do with the natural history of the dayflies as is clearly seen in the Swammerdam picture. The poses of the two figures are in general

FIG. 1. ASSUMED PORTRAIT OF SWAMMERDAM AS DRAWN BY J. STOLKER

so unmistakably similar that I was led to ask of one of my Dutch correspondents if this resemblance had ever been noted. To this question I got an immediate reply that in Holland it had long been recognized that this so-called portrait of Swammerdam was a fraud and had been described as such by J. F. van Someren in the third volume of his *Catalogus van Portretten van Nederlanders* (1891). Here on page 611 this author remarks that the so-called Swammerdam portrait is one of the numerous frauds perpetrated by Jan Stolker who took for it the portrait of

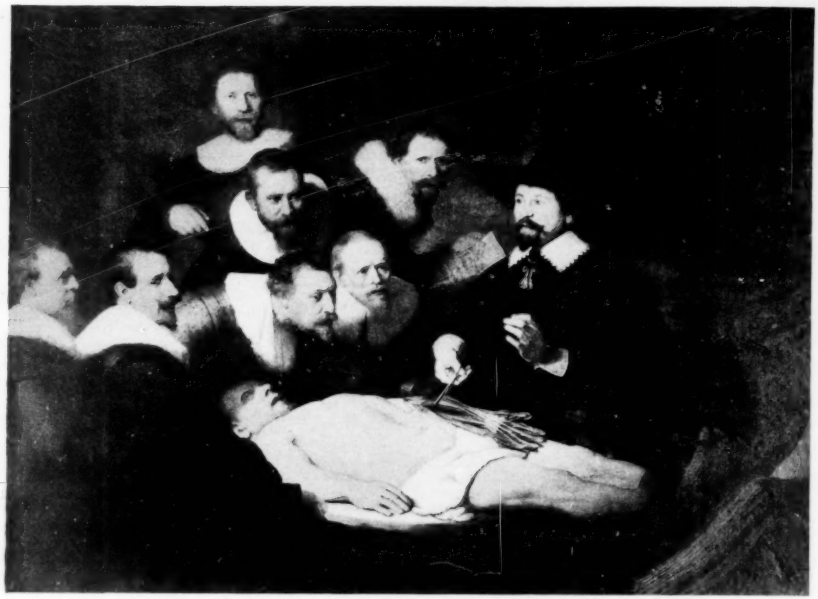


FIG. 2. THE ANATOMY LESSON OF DR. TULP BY REMBRANDT

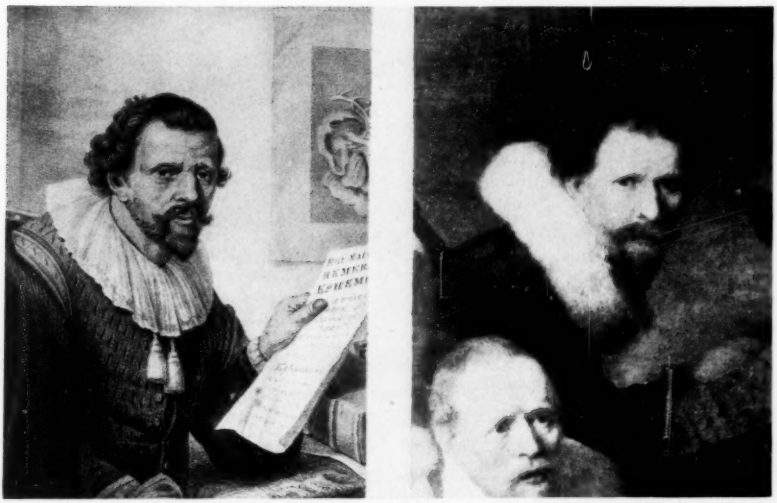


FIG. 3. THE TWO HEADS, ONE THE SO-CALLED PORTRAIT OF SWAMMERDAM (LEFT) AND THE OTHER THE HEAD FROM THE ANATOMY LESSON (RIGHT), ARRANGED FOR COMPARISON

Dr. Matthijs Kalkoen in Tulp's Anatomy Lesson by Rembrandt and by changing slightly the surroundings passed it off for one of Swammerdam. The only additional information on this matter that may be mentioned is that the face in Rembrandt's picture is believed by some not to be that of Dr. Kalkoen but of Dr. Hartman Hartmanszoon. Although the identification of this head may thus not be wholly certain those who have looked into the matter have no hesitancy in pronouncing against Swammerdam as the possible original. It seems extremely probable then that there is no authentic portrait of this highly original but unhappy man and that the so-called picture of him herein reproduced is spurious.

# NEW BIOLOGICAL BOOKS

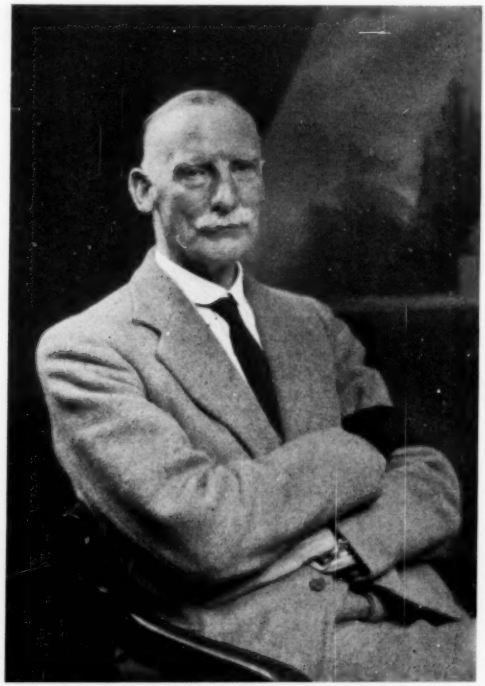
*The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that* THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

## BRIEF NOTICES

### EVOLUTION

EVOLUTION. *Fact or Fiction?* *By E. C. Wren. Thynne and Co., London.* 2s. 6d. $7\frac{1}{4}$ x 5; 107; 1936.

A long time has passed since we have made an addition to our *Fundamentalist Portrait Gallery*. In fact we had about despaired of ever finding another literate fundamentalist. The species seems to be disappearing in America; or perhaps it is only that the New Deal's "more abundant life" has so seduced them that they have



MAJOR E. C. WREN

transferred their major allegiance from Jahweh to Santa Claus. But in England the good old bulldog breed stands firm and solid as Gibraltar in its convictions that the Bible said the last important word about biology, and that biologists are not quite bright.

It is with great pleasure that we present to our readers the portrait of Major E. C. Wren whose *Evolution—Fact or Fiction?* should be in every biological laboratory so that students may learn at first hand the fundamentalist's technique.

Major Wren's little book exemplifies this technique about as well as any anti-evolutionary effort we have ever seen. Its basis is, of course, quotation. You pick out from the writings of eminent scientific persons fragments that, when isolated, appear more than commonly idiotic. Then with righteous complacency you say: "Now then take a look at *that*." As an example which will particularly entertain our American readers for several reasons, we take the following passage from p. 8 of Major Wren's *opus* (the slight inadvertence in spelling is his, not ours):

> There would appear to be a great number of scientists who are against evolution but whose voice is seldom heard, and whose opinions seldom appear in print. Dr. R. A. Millican, the eminent physicist, said, recently, at a meeting of the American Chemical Society, that: "The pathetic thing about it is that many scientists are trying to prove the truth of evolution, which no scientist can do." It is *evidence* the public has a right to demand and should demand before accepting evolution as a science. It is evidence that is so entirely lacking. This lack of evidence is recognized by many evolutionists who only cling to evolution as an act of faith, hoping that some day some evidence may be forthcoming.

Indubitably our author has done a great deal of reading over a wide range, and has missed few morsels suitable for the nourishment of his literary baby. We were strongly tempted to make a statistical table showing the names of the persons he quotes and the frequency of occurrence of each name. The table would be impressive but the labor involved appeared too great for the result to be achieved. It would, however, make clear the range of Major Wren's preparation of his case.

One more quotation will illustrate his agility as a debater.

> It is well to know that Sir Arthur Keith has said: "as our knowledge increases the vestigial organs decrease," and "we find the same vestigial structures—the same evolutionary postmarks—in the body of man and anthropoid." The "vestigial hairs" of the human embryo are supposed to be proof of his anthropoid ancestry. The monkey embryo passes through a hairless stage, and it is only later that it grows the hairy coat with which it enters the light of day. We do not hear that this hairless stage is a "vestigial" stage proving the monkey's human ancestry. The tissues which produce the "hormones," those remarkable, mysterious, and little-understood substances, have been called "vestigial" and useless by Darwinians. They are now known to be indispensable to life. Sir Arthur Keith says of these "hormones," the product of those "vestigial" organs, that they are "Chemical substances in ultra-microscopic amounts, dispatched from one community to another in the circulating blood. Clearly a discovery of this ancient and intricate system opens up fresh vistas to the student of man's evolution. How Darwin would have welcomed this discovery." Not long ago when the tissues producing hormones were called vestigial they were claimed as good evidence in favour of evolution; to-day when they are found, after all, not to be vestigial but, on the contrary, essential to life they are just as good evidence for evolution as they were before.

We think it is a mistake to suppose, as too many biologists are apt to do, that fundamentalists are intellectually dishonest when they use this sort of argument. Some of them doubtless are, but equally some are not. We are convinced in this particular case that Major Wren is a thoroughly honest man, but terribly misguided because of his profound ignorance of biology. We feel sure that he sincerely regards such points as those we have quoted as real and significant evidence against the occurrence of organic evolution as a fact of nature. Which brings us to the reiteration of the statement made at the beginning: namely that this book should be in every biological laboratory so that students by reading it can really understand, in the light of their first-hand knowledge of the data of biology, how intrinsically feeble and far removed from reality are the arguments brought against the idea of evolution, and what a difference it makes on one's outlook really to have some precise and ordered knowledge of biology and the facts of nature generally.

PROBOSCIDEA. *A Monograph of the Discovery, Evolution, Migration and Extinction of*

*the Mastodonts and Elephants of the World. Volume I: Moeritherioidea, Deinotherioidea, Mastodontoidea.*

*By Henry Fairfield Osborn. Edited by Mabel R. Percy. The American Museum Press, New York.* $20. $12\frac{1}{2}$ x $10\frac{3}{4}$; xl + 802 + 2 folding charts; 1936.

This magnificent and fundamental contribution to the facts and theory of organic evolution will stand as the final and fitting monument to the genius of Henry Fairfield Osborn. This first volume only half completes the work, but the second and concluding volume is promised for the current calendar year.

The most striking characteristic of the monograph is its thoroughness—a feature that marked all of Professor Osborn's paleontological work. Next is the wealth, precision, and beauty of the illustrations. Here, once more, is biology in the grand manner that we see so little of today in a world lavishly extravagant in wasting money on ephemeral enterprises, and excessively parsimonious in supporting enterprises of permanent worth to mankind. Great sections of biology still are, and will long remain, necessarily descriptive in their technique of investigation and for these regions adequate illustration is an absolute and ineluctable necessity. Precise and comprehensive descriptions of the world of living things—past and present—still constitute one of the major needs of biology as a science.

The significance of this monograph for students of evolution cannot be better indicated than by quoting Professor Osborn's own estimate of a part of it.

> Not revolutionary, but in accord with the clocking of geologic time by similar stages of evolution (homotaxis, Huxley) long in use by invertebrate palaeontologists, is the evidence yielded by intensive examination which the Proboscidea afford for Tertiary geologic correlation of stages in adaptive progression and retrogression in the widely distant distribution centers of mastodonts and elephants which migrated into all the continents except Australia. The continuous evolution of far-separated adaptations of the grinding teeth, in northern and southern Africa, in Eurasia and in North and South America, can now be coördinated with unexpected precision, as in the outstanding metamorphosis of *Archidiskodon proplanifrons* of South Africa into the gigantic *Archidiskodon maibeni* of Nebraska. In this correlation the intensive study of the grinding teeth in the mastodontoid and elephantoid divisions plays a leading part.
>
> Another result which will prove to be revolutionary in anthropology is the new means afforded of dating precisely the main periods of the prehistoric evolution of Man, by intensive measurement of the length, width and thickness of the enamel layers in the grinding teeth of the elephantoid division in successive stages from Upper Pliocene to closing Pleistocene time. Man was a mastodon and elephant hunter from early times. Remains of fossil elephants are occasionally found embedded in the same strata with remains of fossil man, and the total length of the enamel foldings in proboscidean grinding teeth enables us to date relatively the successive phases in the evolution of man.
>
> From the author's prolonged thirty-five-year research on Titanotheres and Proboscidea there issue not only the principles governing the classic modifying modes of evolution known to Lamarck and Darwin (variation, development, degeneration) but also the newly discovered and hitherto unrecognized principle and modes of the origin of new characters through aristogenesis or creative biomechanical rectigradation.
>
> Ascending mutations, species and genera are principally defined by continuously progressive changes in the aristogenes and proportions of the grinding teeth and in the upper or lower incisive tusks. Although the rate of proportional adaptation varies enormously, for the first time we know approximately how long a period of geologic time it takes to produce a full-fledged and highly efficient adaptation, as in the metamorphosis of the lower incisive tusks of Oligocene *Phiomia* into the dominant shovel-tusks of Pliocene *Amebelodon*, or in the metamorphosis of the posterior grinders of *Trilophodon* from Lower Miocene into Mio-Pliocene.
>
> Through the clear distinction between change of proportion (alloiometry) and the origin (aristogenesis) of new parts, also through the newly discovered multiple lines (forty-one or more) of ancestry and ascent technically known as phyla, the Proboscidea afford a complete revolution in our biological philosophy and concepts of the nature and causes of evolution.
>
> Never before has it been possible to follow many lines of phylogenetic ascent over extremely long periods of geologic time, noting the progressive adaptive changes in each organ in each phylum to gain the perfection of certain mechanisms at the expense of other mechanisms. In general, the specialization of certain organs becomes more intense, while closely contiguous organs remain absolutely stationary. For example, among the shovel-tuskers, *Phiomia osborni* of the Oligocene of Egypt gives rise to the incredibly specialized *Amebelodon fricki* of Nebraska, with a relatively similar and unchanging skeleton and limbs. *Amebelodon* is paralleled by the flat-tusker *Platybeledon* of the Gobi Desert and of Nebraska in which the whole jaw becomes an enormous shovel, the upper jaw and skull being sacrificed and thus greatly reduced in size.

It gives us great pleasure that an accident of editorial timing makes it possible for us to place side by side this and the preceding review. So long as biologists continue to pile up the kind of real

evidence for organic evolution that Osborn's Proboscidea does, the world has no reason to worry over the activities of the fundamentalists.

## GENETICS

EUGENICAL STERILIZATION. *A Reorientation of the Problem.*

*By The Committee of the American Neurological Association for the Investigation of Eugenical Sterilization: Abraham Myerson, James B. Ayer, Tracy J. Putnam, Clyde E. Keeler and Leo Alexander. The Macmillan Co., New York.* $3.00. 8½ x 5½; xii + 211; 1936.

By an ample margin this volume stands in the front rank in the eugenic field of discourse for its scientific soundness, unemotional sanity, and plain common sense. After pointing out that the "most of the legislation [for eugenic sterilization] which has been enacted so far is based more upon a desire to elevate the human race than upon proven facts," the Committee makes the following recommendations:

*First.* Our knowledge of human genetics has not the precision nor amplitude which would warrant the sterilization of people *who themselves are normal* in order to prevent the appearance, in their descendants, of manic-depressive psychosis, dementia praecox, feeblemindedness, epilepsy, criminal conduct or any of the conditions which we have had under consideration. An exception may exist in the case of normal parents of one or more children suffering from certain familial diseases, such as Tay-Sachs' amaurotic idiocy.

*Second.* Particularly do we wish to emphasize that there is at present no sound scientific basis for sterilization on account of immorality or character defect. Human conduct and character are matters of too complex a nature, too interwoven with social conditions, such as traditions, economics, education, training, opportunity and even prejudice, especially when these factors operate in the earlier years of life, to permit any definite conclusions to be drawn concerning the part which heredity plays in their genesis. Until and unless heredity can be shown to have an overwhelming importance in the causation of dangerous anti-social behavior, sterilization merely on the basis of conduct must continue to be regarded as a "cruel and unusual punishment."

*Third.* Nothing in the acceptance of heredity as a factor in the genesis of any condition considered by this report excludes the environmental agencies of life as equally potent and, in many instances, as even more effective. That scientific day is past when the germplasm and the environment are to be considered as separate agencies or as opposing forces. Both operate in the production of any character, though in different degrees, but the degree in which each operates is, at present, mostly in the field of the unknown.

The book records an important job of work well done. We congratulate the American Neurological Association on their acumen in promoting the inquiry and picking the men to carry it out.

The book is well planned and simply written without undue technical rumble-bumble. The bibliographic documentation runs to 20 pages. There are author and subject indexes.

YEARBOOK OF AGRICULTURE, 1936.

*U. S. Department of Agriculture. Government Printing Office, Washington.* $1.25. 9 x 5¾; 1189; 1936.

AGRICULTURAL STATISTICS, 1936.

*U. S. Department of Agriculture. Government Printing Office, Washington.* 50 cents. 9¼ x 5¾; 421; 1936 (paper).

This year, instead of the usual "brief summaries of miscellaneous new developments in agriculture," the *Yearbook* presents a single subject: "the creative development of new forms of life through plant and animal breeding." The material contained in the volume is the result of a survey made by the Committee on Genetics. It consists chiefly of a discussion of specific improvements and general progress made in 19 major crops and types of livestock (wheat, cotton, sugar beets, cattle, swine, etc.). In addition, however, the reader is introduced into the general field of genetics and made aware of the importance of this subject in sections on Better Plants and Animals; Heredity Under the Microscope; and Unusual Possibilities in Breeding.

Technical terms have been eliminated wherever possible, but since in some cases it was necessary to use them, a glossary is provided. Each section has been written by a specialist and each represents a comprehensive survey of its subject. The text is illustrated with tables and photographs and at the end of each section there is a list of the literature cited. Anyone interested in breeding, whether

commercial or experimental, will find much interesting material in this volume.

The volume of *Agricultural Statistics* contains information which, until this year, was published in the statistical section of the *Yearbook of Agriculture*. The usual "most important agricultural statistics of the United States and of the world so far as the agriculture of this country is concerned" are presented.

## GENERAL BIOLOGY

EXPERIMENTELLE BEITRÄGE ZU EINER THEORIE DER ENTWICKLUNG. *Deutsche Ausgabe der Silliman Lectures Gehalten an der Yale University im Spätjahr 1933.*

*By Hans Spemann. Julius Springer, Berlin.* RM. 27 (paper); RM. 29.60 (cloth). $9\frac{3}{4}$ x $6\frac{1}{4}$; viii + 296; 1936.

This monograph, a German translation of a series of lectures delivered at Yale University in 1933, contains an excellent summary of many experimental embryological studies including remarks on the technique involved; a lengthy discussion of the relation of these studies to the problems of ontogeny, and a splendid bibliography of both recent and classical titles. The volume is especially noteworthy in its attempt to weave a great mass of isolated facts into a general picture of development the central key of which is the conception of embryonic induction. The author emphasizes primarily experiments in which a controlling region or "organizator" in the embryo is shown to play a definite rôle in shaping differentiation by guiding the pattern of growth.

This book by a distinguished author is a tribute to its field and merits the serious attention of all biologists. A number of illustrations supplement the text. There is no index.

Организация Клетки

Н. К. Кольцов. Государственное Издательство биологической и медицинской литературы, Москва-Ленинград. 14. руб.; 652; 1936 [THE ORGANIZATION OF CELL.

*By N. K. Koltzoff. State Publishing House for Biological and Medical Literature, Moscow-Leningrad.* 14 roubles; 652; 1936 (cloth)].

In this volume, dedicated to the coming centenary of the cell theory, are collected papers published by Professor Koltzoff in the course of the last thirty-three years. Practically all of them were published in the past in the two languages, Russian and German, and in this way made available to the biological world.

These collected papers, representing the life-work of one of the leading Russian biologists, give an impressive account of fruitful attempts to apply the methods and modes of thought of physics and chemistry to cytology and biology in general. Having started his career as a comparative anatomist, Professor Koltzoff became soon interested in experimental cytology and published a number of interesting papers on cellular morphology from a physicochemical viewpoint. In the Institute of Experimental Biology, organized by him in Moscow, he has for many years cultivated cytology, physiology of development, and genetics. Many excellent articles have issued from this Institute, and many of his students and associates, particularly in genetics, have now become leading Russian scientists with world-wide reputations.

OCTOBER FARM. *From the Concord Journals and Diaries of William Brewster.*

*Edited by Smith O. Dexter. Introduction by Daniel C. French. Harvard University Press, Cambridge.* $2.50. $7\frac{1}{2}$ x $5\frac{1}{4}$; xv + 285 + 5 plates; 1936.

This book consists of excerpts from Brewster's voluminous diaries, kept during practically his whole life. The material extracted deals particularly with observations about Concord where the author lived on his home, October Farm. There is a forword by T. Barbour and a partly biographical introduction by Brewster's life long friend, Daniel Chester French. For once we feel that we have read a diary that was not written with an eye to publication. In a simple, unaffected and quite unexciting style the author describes the country scenes and wild animals he observed over a period of forty-seven

years in his daily wanderings about his farm.

STUDIEN ÜBER DAS ZUSAMMENSPIEL VON HYPOPHYSEN- UND OVARIALHORMONEN, INSBESONDERE IM LICHTE VON PARABIOSEVERSUCHEN.

*By Ejnar Møller-Christensen. Levin and Munksgaard, Copenhagen.* 10 x 6½; 157 + 3 plates; 1935 (paper).

The first part of this book is a brief résumé of earlier work on the effects of hypophysectomy on the sexual apparatus and function. In the experiments discussed in detail in the second part it was demonstrated that the oestrus cycles of two normal, adult female rats ran independent courses during parabiosis, and likewise that such union between normal males and normal females effected no changes in the organs or functions of either. However, in parabioses between castrated and normal animals, the hypophysis of the normal animal exercised a retarding effect on the oestrin activity and ovarian cycle of that animal. The ovaries, uterus and oestrus cycle remained normal in the normal rat during parabioses of hypophysectomized and normal female rats.

Each part is followed by a bibliography. The illustrations include text figures and three colored plates.

OUR NATURAL RESOURCES AND THEIR CONSERVATION.

*Edited by A. E. Parkins and J. R. Whitaker. Contributions by various authors. John Wiley and Sons, New York; Chapman and Hall, London.* $4.00. 9 x 5¾; xii + 650; 1936.

A general survey of the entire field of our natural resources is presented here through the coöperation of twenty-two authors. The major topics selected for discussion are soils, forests, water and minerals and the various problems relating to these resources and their conservation. The extent and distribution of each resource and its service in national and regional development is discussed and the degree of exploitation considered. There are chapters of general and economic interest on the agricultural outlook; the conservation of natural resources in relation to the manufacturing industry; and on "The Conservation of Man." Lightly touched upon are problems relating to private versus state or federal control of the conservation and exploitation of some of the natural resources. A bibliography and index are included.

ENTWICKLUNGSBIOLOGIE UND GANZHEIT. *Ein Beitrag zur Neugestaltung des Weltbildes.*

*By B. Dürken. B. G. Teubner, Leipzig and Berlin.* RM. 6.80 (cloth); RM. 5.80 (paper) (in Germany). RM. 5.10 (cloth); RM. 4.35 (paper) (outside of Germany). 9 x 6; vi + 207; 1936.

This work by the Director of the Institute for the Experimental Analysis of Development and Heredity at the University of Breslau gives us a comprehensive exposition of the biology of development from the viewpoint of biological holism. Of strictly scientific character and at the same time written in a lucid style, provided with numerous illustrations and elucidating notes together with references to the literature on the subject, it will prove of great value not only to the professional biologist but also to the educated layman interested in the modern trends of scientific research. A list of explanations of the most important technical terms and an index of names and topics are provided.

URDEUTSCHLAND. *Deutschlands Naturschutzgebiete in Wort und Bild. Lieferungen* 21, 22, 23.

*By Walther Schoenichen. J. Neumann, Neudamm.* 2 marks each. 10¾ x 8¼; Lief. 21, 193–216 + 9 plates; Lief. 22, 217–240 + 9 plates; Lief. 23, 241–280 + 9 plates; 1936 (paper).

The latest numbers of a publication, already mentioned frequently in this journal. Parts 21 and 22 continue with the account of Parks begun in Part 20, and then deal at greater length with marsh lands of North Germany, and the plant and animal life found in them.

Part 23 deals with the heaths. All three sections contain many beautiful full page illustrations.

TRANSZENDENTALE GRUNDLAGEN DER BIOLOGIE.

*By Erich Oelze and Otto Schmith. Johann Ambrosius Barth, Leipzig.* RM. 1.80. $8\frac{1}{4}$ x $5\frac{7}{8}$; 36; 1937 (paper).

A philosophical analysis of the fundamental concepts and presuppositions of biology and a critical examination of the theories of monism, mechanism, holism and vitalism in the light of Kantian epistemology. The worthy aim is to establish the science of biology upon a sound rational basis.

PRACTICAL PHOTO-MICROGRAPHY.

*By J. E. Barnard and Frank V. Welch. Longmans, Green and Co., New York; Edward Arnold and Co., London.* $8.25. $8\frac{1}{2}$ x $5\frac{3}{8}$; xii + 352 + 23 plates; 1936.

This text is practical in fact as well as in name, being clearly written and not too technical for those who wish to use photo-micrography in their work without going into it as a separate science. In the third edition more attention has been given to the photography of metal surfaces. Sections have been added on the subjects of infra-red and ultra-violet photography.

THE MENDEL BULLETIN, *Volume IX, Number I.*

*Villanova College, School of Science, Villanova, Pa.* 35 cents a copy; $1.00 a year. $9\frac{1}{2}$ x $6\frac{3}{4}$; 28; 1936 (paper).

This is the first number, in new format and attractively printed, of a quarterly that had hitherto been an annual.

## HUMAN BIOLOGY

THE PURITAN PRONAOS. *Studies in the Intellectual Life of New England in the Seventeenth Century. By Samuel E. Morison. New York University Press, New York; Humphrey Milford, London.* $3.75. $9\frac{1}{4}$ x 6; x + 281; 1936.

Religion is usually considered the most important factor in the intellectual life of Puritan New England. In this book the author emphasizes also the interest in the humanities, which included secular poetry and the classics, and in science. The establishment of elementary schools, public grammar schools and Harvard College so soon after the arrival of the colonists in America indicates the importance they attached to education. When political and historical literature appeared, it was made available by printers and book sellers, and preserved in the libraries. The overwhelming importance of religion in the Puritan civilization is brought out, and the sermon is discussed both as a literary form and as theological exposition.

In providing institutions that later generations could put to new uses, the New Englanders were more successful than in actual intellectual production. No lasting books were written, no new or significant ideas were worked out on New England soil; but three institutions of lasting significance for American life were firmly established: the college, the public-school system, and the Congregational Church. The story of the intellectual life of New England in the seventeenth century is not merely that of a people bravely and successfully endeavoring to keep up the standards of civilization in the New World; it is one of the principal approaches to the social and intellectual history of the United States. Primitive New England is a puritan pronaos to the American mind of the nineteenth century, and of today.

THE CRADLE OF MANKIND. *Life in Eastern Kurdistan. Second Edition. By W. A. Wigram and Edgar T. W. Wigram. A. and C. Black, London; The Macmillan Co., New York.* $4.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xi + 430 + 16 illustrations + 1 folding map; 1922.

This is an altogether remarkable book. The country with which it deals lies roughly between the easternmost end of the Mediterranean and the southwestern border of the Caspian Sea, and two or three hundred miles south and a little east of the southern border of the Black Sea. More precisely it lies for the most

part between the upper reaches of the Euphrates and Tigris rivers. It is a country of wild and magnificent scenery. It contains some of the most venerable monuments in the world. "It is the very *fons et origo* of our Indo-European ancestors. Its traditions connect it with the Garden of Eden, with Noah, and with Abraham. Its folk-lore preserves the old Nature-worship which originated in the brains of the Apeman. Its history records the very dawn of civilization, and the rise and fall of the earliest of the great empires."

What makes this book so extraordinarily interesting is fundamentally the fact that the every-day life of the present inhabitants remains to this moment in all essential details the life of Europeans in the Dark Ages—a sort of living paleontological record of human biology. The country is extremely inaccessible and does not welcome visitors gladly—quite the contrary in fact. "Never another law hae they but the length of their dirks; the broad-sword's pursuer, and the target is defender, and the stoutest head bears langest out."

The senior author lived in Eastern Kurdistan ten years as a member of the "Archbishop of Canterbury's Assyrian Mission," which exists at the request of the Patriarch and other authorities of that most ancient communion, the Nestorian Church. He quite evidently got the complete confidence, respect and affection of the people, as his understanding humanity, tolerant good humor, and native wit obviously fitted him to do. He writes simply and with great charm. The result is, as we have said before, a very remarkable book. We recommend it in the highest terms.

AFRICA'S GOD. III. *Nigeria. Anthropological Series of the Boston College Graduate School, Volume 1, Number 3.*

*By Joseph J. Williams, S. J. Boston College Press, Chestnut Hill, Mass.* $1.00. 9¾ x 6½; 56; 1936 (paper).

AFRICA'S GOD. IV. *French West Africa. Anthropological Series of the Boston College Graduate School, Volume 1, Number 4.*

*By Joseph J. Williams, S. J. Boston College Press, Chestnut Hill, Mass.* $1.00. 9¾ x 6½; 32; 1936 (paper).

Nigeria lies on the west African coast just north of the equator. The author attempts to show that the bronzes of Benin actually antedate the advent of the Portuguese to West Africa, and that they are closely associated with their religious beliefs. He quotes a few earlier accounts of coastal Nigeria and passes on to a consideration of modern narratives. He also takes up answers to his questionnaire which was originally sent out in 1932. The last part of the monograph is an account of Stephen Septimus Farrow who has from personal experience delved into the esoteric secrets of the Yorubas as a people. In conclusion, the religious status of the Nigeria tribes is less clearly defined than the author's findings for other tribes in previous monographs. He claims, however, that there is unquestionably a Supreme Being extant among them.

In French West Africa, just north of Nigeria, there is further retrogradation due to Mohammedan influences. The author finds a decadent form of monotheism existent among the pagan tribes. They offer no cult to the Supreme Being, but they still recognize Him as the Creator.

Both monographs are carefully planned and well written. They each have long bibliographies and two alphabetical indices for persons and topics discussed.

THE LIFE AND CONVICTIONS OF WILLIAM SYDNEY THAYER, PHYSICIAN.

*By Edith G. Reid. Oxford University Press, New York.* $2.50. 8½ x 5½; xii + 243; 1936.

Although less publicized than some of the early Hopkins medical luminaries, William Sydney Thayer certainly deserves, on both professional and personal grounds, a biography. It is eminently apparent that his biographer, Edith Gittings Reid, also is of the same opinion for one notes patent and not always subtle evidences of "hero-worship" even before the first page has been turned. This attitude persists throughout the volume but, happily, is kept from being irritating by the genuine

enthusiasm of the author for her subject and by the introduction into the text of numerous interesting anecdotes. The reviewer, not having had the privilege of knowing Doctor Thayer personally, is unable to appraise critically the character analysis developed in the book. It is felt, however, that an interesting and commendable job of reporting is done that results in a portrait of a man artfully skillful as a diagnostician and successful as a practising clinician; sartorially impeccable; rock-bound New Englandish (and proud of it); militaristic and completely the gentleman. Not being a medical person herself the author wisely refrains from entering into research or technical discussions, but nevertheless succeeds in turning out a pleasant little volume that will be especially welcome around Baltimore and other American medical centers. The book has an index and an interesting outline-log of Thayer's life including such memorabilia as collegiate degrees, positions, society memberships, editorships, and world-war records. Several photographic studies accompany the text.

AUDUBON.

*By Constance Rourke. Harcourt, Brace and Co., New York.* $3.00. $8\frac{3}{4}$ x $6\frac{1}{4}$; 342 + 12 plates; 1936.

For years a mild interest has centered about the personage of John James Audubon. Recently, when Donald Culross Peattie in his excellent book *Green Laurels* compared Audubon with Wilson (somewhat to the former's disadvantage) a new interest in the man and his works cropped up. Peattie, having many illustrious naturalists to epitomize, touched on Audubon only briefly and provocatively. He succeeded, however, in unconsciously setting the stage for a more extended biography which has been attempted in the present book.

The author, Constance Rourke, is well-known as an interpreter of frontier Americana and approaches Audubon, his peregrinations and his accomplishments from this viewpoint. The book, after briefly reviewing Audubon's boyhood in France, soon carries him to America. There he engaged in commerce and amateur ornithology—the former with tepid success—the latter with growing artistic brilliance. In the remaining pages Audubon's character, matrimony, personal quirks, woodland jaunts and bird studies are expounded. The author succeeds in making all these stories interesting, and, although the book is not a penetrating character study, it does fill competently a niche in the shelves of American biographies and, as such, should enjoy considerable popularity. The text is illustrated with 12 colored plates taken from original Audubon prints and has an index and annotated bibliography.

MAN MAKES HIMSELF.

*By V. Gordon Childe. Watts and Co., London.* 7s. 6d. net. $7\frac{3}{4}$ x 5; xii + 275; 1936.

One of the most distinguished authorities on the archeology of Europe and the near East carries the arguments and evidences of the economic interpretations of history into the fertile field of prehistory. Avoiding controversial points and "high sounding terms that give confusion the semblance of logic," Professor Childe discusses the origin and development of the more important cultural traits upon which civilizations are based—stressing the material traits but including those of social organization and religion—from Paleolithic times to the dawn of recorded history. The rôles of diffusion, environment (raw materials, geographic position, climate, etc.), race and other factors contingent upon the growth of culture are considered not in contrast to economic factors, but as intimately and inextricably bound with them. Of particular interest to human biologists are his evidences of the phenomenal increase in population following upon the first two revolutions—the Neolithic Revolution, which was based upon the production of food through domestic plants and animals in contrast to hunting and collecting; and the Urban Revolution, attendant upon the inventions of the techniques of metallurgy and involving highly developed trade, specialized craftsmen, etc. These population

spurts closely parallel the well known one connected with the third or Industrial Revolution. In easy style and with masterful command of the data of prehistory, Professor Childe demonstrates "how economic revolutions . . . promoted the growth of institutions, science, and literature—in a word, of civilization as currently understood."

THE MYSORE TRIBES AND CASTES. *Volume I. By L. K. Ananthakrishna Iyer. The Mysore University, Mysore.* Rs. 15 or 24s. 8½ x 5½; lxxii + 205 + 66 plates + 2 folding maps; 1935.

While this is indicated as Volume I, actually it is the fourth and last of a series of publications to report on a detailed ethnographic survey of Mysore. This country which has a population of about 5 million and an area of almost 30,000 sq. miles is second in importance only to Nizam among the Indian States. Situated as it is, near the southern tip of India, it has been touched by practically all the cross-currents of population movements which have taken place in India since time immemorial. As a result the anthropologist finds here an interesting conglomeration of different ethnic groups, some of which have modified considerably while others maintained their original customs. In this volume, a summary of the preceding ones, the author first describes in detail these historical movements and then discusses the effects on the original mores due to contact and diffusion. The origin of castes is one of the principal questions on which he attempts to throw some light. He recalls how they are already mentioned in the Rig Veda and outlines the important factors which have favored the formulation and maintenance of such endogamous groups: contact with foreign peoples, similarity of occupation, of culture and of religion. In some detail he compares the customs and beliefs held by the different tribes and castes. The book closes with an appendix containing a most interesting description of the mores of six tribes whose main occupation is thievery.

It really seems unnecessary to add that this is a most important contribution to anthropology for the wealth of material which it contains and for the scholarly treatment of the subject.

THE AMERICAN PEOPLE. *Studies in Population. The Annals of the American Academy of Political and Social Sciences, Volume 188. Edited by Louis I. Dublin. The American Academy of Political and Social Science, Philadelphia.* $2.50 (cloth); $2.00 (paper). 9¼ x 6¼; xii + 396; 1936.

A group of America's most eminent demographers have contributed to the special population number of this journal. Its purpose, as Dublin states, is to exhibit the present status of population studies in this country and from this summary possibly develop an intelligent and consistent point of view towards the general problem of population. The articles, 33 in number, are distributed in 6 sections which treat, respectively, of reproduction, morbidity, population increase and structure, relation of population to resources, world population problems and organization of population research. A few of the articles present original data but the remainder simply provide excellent and scholarly summaries of the known facts regarding each particular topic. A number of authors conclude with a lamentation about the scarcity of data and with a plea for more and more accurate information. This is necessary, but it is doubtful whether the lack of data is alone responsible for the general sterility exhibited by some of these articles. Demography is at bottom only a branch of human biology and until it is well founded on biologic bases its study will generally consist of periods of preoccupation about surplus population and periods of preoccupation about insufficient population.

THE STRUGGLE FOR POPULATION. *By D. V. Glass. With an Introduction by A. M. Carr-Saunders. Oxford University Press, New York; Clarendon Press, Oxford.* $2.75. 8¾ x 5½; x + 148; 1936.

This is the outcome of an inquiry undertaken by a select Committee under the

chairmanship of Professor Carr-Saunders set up by the Council of the Eugenics Society (English). The main idea was to find out why the birth rate was declining and what had best be done about it. The net upshot of the inquiry—after reaching the sound conclusion that an increase in contraception and abortion is not a quite complete or satisfactory explanation of slowing rates of population growth—is stated in the following words:

> Our conclusion is, then, rather important even though it is not positive. What seems imperative is, first, a series of detailed studies of movements within the population of this country, and, secondly, a careful analysis of the factors which are urging people to keep down the size of their families. Until this is done, attempts to raise the birth-rate will be so much struggling in the dark with small chance of success. There are only two points on which we may be fairly positive at present. First, repressive measures are unlikely to be effective; what appears to be much more necessary is the creation of a general environment conducive to the bringing up of relatively large families. Secondly, if there is to be any significant increase in the birth-rate, the major part must come from the working-class. Consequently, no action is likely to have a permanent influence unless it provides conditions in which the working-class is able to bring up children without thereby suffering from economic and social hardship.

CUSTOM IS KING. *Essays Presented to R. R. Marett on his Seventieth Birthday, June 13, 1936.*

*Edited by L. H. Dudley Buxton. Hutchinson's Scientific and Technical Publications, London.* 12s 6d. 8½ x 5½; xiii + 325 + 4 plates; 1936.

This volume contains a series of 19 essays written by students and colleagues of Professor R. R. Marett as a tribute for his seventieth birthday. While the articles cover a rather wide range of subjects in the field of cultural anthropology and ethnology the majority of them adhere to the general plan of the book as indicated by the title, and report observations on the customs of peoples. The two subjects mainly discussed are religious beliefs and intra-tribal relationships. Thus there are articles on totemism in West Africa and along the north-west coast of North America, kinship, incest and exogamy in the northern territories of the Gold Coast, bond friendship in Tikopia, the Wiro sky-god, the chameleon and the sun-god Lisa on the West-African slave coast, the Paradise myth, religion in south-eastern Asia. There are, besides, papers dealing with such generalities as the Western seaways, snobbery and the relation of physical anthropology to cultural anthropology. Except for the last two articles mentioned which are of a philosophical nature, the remaining are characterized by a clear description of observations and little or no speculation. All are rather brief but well written. The book contains also a short sketch of the scientific career of Professor Marett and a bibliography of his scientific writings.

COMTE. *The Founder of Sociology.*

*By F. S. Marvin. Chapman and Hall, London.* 6s. net. 7¼ x 4⅞; 216; 1936.

The author introduces this book with the question: "Can Comte really be called a philosopher?" and dedicates the chapter to prove the affirmative. It is Comte, the philosopher, the idealist, the metaphysician, who is important in the author's view and it is primarily from this standpoint that he evaluates the contribution of the father of Positivism. This volume contains a brief sketch of Comte's rather sad existence, his social and domestic environment, and the influence of St. Simon. The author emphasizes Comte's dream of a regimented universe, under the leadership of a scientist, in which humanity would not suffer and in which there would be universal peace. Unless the author is deliberately courting the favor of pacifists *et similia*, it is not readily understood why he returns to this point again and again while at the same time he is forced to defend the fact that Comte's hopes for establishment of Positivism never came true. The author goes so far as to state that the League of Nations, "the largest new definite factor in the world" (sic) is already an approach to the ideal of Comte. There is more in Comte's sociology than "humanity," "faith," etc. and it is a pity that the author should have made him into a mystic.

THE CLEAR MIRROR. *A Pattern of Life in Goa and in Indian Tibet.*

*By G. Evelyn Hutchinson. The University Press, Cambridge; The Macmillan Co., New York.* $2.50. 8¾ x 5½; xi + 171 + 13 plates; 1936.

This book is disappointing. It does not give us what the subtitle promises—a pattern of life in Goa and in Indian Tibet. The author uses as a symbol a Spanish mirror to which is fastened a figure of the Madonna. Spain and Portugal sent out "a spiritual army entrusted with placing the Madonna in front of every strange and barbarous culture in which could be recognized the distorted image of her own." But then he does not interpret the civilizations of which he writes in accordance with this idea.

The section on Goa is reminiscent of a guide book with the exception of a description of the feast of St. Catherine. In the part on Tibet, the paintings in the temples are described and an account of the Black Hat Dance is given. The last section is devoted to the numerous lakes of Tibet and the forms of life found in them.

It is obvious that the author is an observant traveler, but his book is not very interesting.

THE GENTLE SAVAGE. *A Sudanese Journey in the Province of Bahr-el-Ghazal, commonly called "The Bog."*

*By Richard Wyndham. William Morrow and Co., New York.* $2.75. 8½ x 5¾; 287; 1936.

The author—an English artist—writes with an ease not common to all, and in this interesting story of a three months' sojourn in "the Bog" of Anglo-Egyptian Sudan he has demonstrated an ability to portray vivid word pictures just as he can produce with paint and brush those pictures for which he is better known. He is quite naturally interested in the natives as models for his canvas, but his interests are by no means limited to this feature. He finds pleasure in observing odd characters, the traits and characteristics of the various tribes he comes to know and in particular of those individuals with whom he is most closely associated. Two young girls selected for models had to be purchased with cattle since their parents refused to see the difference between a model and a wife. Accounts of curious scenes, minor adventures, and details of everyday life among these black people are all woven into the fabric of the tale. The photographs with which the book is illustrated are artistic and extremely well reproduced.

OPPORTUNITIES FOR THE MEDICAL EDUCATION OF NEGROES.

*By E. H. L. Corwin and Gertrude E. Sturges. Introduction by Walter L. Niles, and a Foreword by Walter White. Charles Scribner's Sons, New York.* $1.50. 7½ x 5¼; xv + 293; 1936.

In 1920 the first appointment of a Negro physician was made at Harlem Hospital—a city hospital situated in the heart of the Negro district of New York. In 1933 there were 27 Negroes on the indoor staff. But during this period there was considerable agitation over the appointments made. One faction opposed the appointment of graduate students of Negro medical colleges and approved only of those Negroes who had competed successfully with whites in the northern medical schools of high standing. This brought to a head a consideration of the problem of opportunities for advancement in the medical education of those graduates of Negro institutions who are eliminated through racial prejudice from practice in practically all large hospitals. This book is the report of a critical study made of Harlem Hospital by a group of medical experts and laymen from a point of view of its peculiar fitness for the further training of all qualified Negro doctors. A brief account is also given of the Negro medical, hospital and health situation in the country at large.

PLASTIQUE MAMMAIRE. *Considérations médico-chirurgicales.*

*By Irène Bernard. Librairie Maloine, Paris.* 40 francs. 10 x 6½; 245; 1936 (paper).

The pamphlet *L'Esthétique mammaire à travers l'histoire* by C. Claoué and I. Bernard, noticed in the immediately preceding number of the Q. R. B. is reprinted here to serve as the first chapter. Then follows a description and evaluation of the various measures which have been tried in France in an effort to make the feminine breasts conform to the modern ideal of beauty. The operative procedure of Claoué, under whom the author worked, is recommended on the basis that no scars are left on throat or arm-pits. This is undoubtedly an advantage.

The illustrations consist largely of photographs and diagrams of stages in operative procedure used by the leading plastic surgeons, and "before" and "after" pictures. The bibliography is confined to French titles, from which, curiously enough, references to the writings of the men whose methods have received the greatest discussion in the text, with the single exception of Claoué, are omitted.

STONE-AGE BUSHMEN OF TO-DAY. *Life and Adventure among a Tribe of Savages in North-Western Australia.*

*By J. R. B. Love. Blackie and Son, London and Glasgow.* 8s. 6d. net. 8½ x 5½; xxiii + 220 + 16 plates; 1936.

This book is an account of the life and customs of the Worora, a tribe of savages in North-Western Australia, and of the author's experiences among these people. J. R. B. Love, a missionary, studied these aborigines and learned their language so that he might really understand them. In this book we receive the full benefit of his work, for he has given us a detailed and interesting picture of these primitive men.

At the end of the volume Mr. Love says, "the gulf between the most highly civilized man and the most savage is far less than many a casual thinker believes." And the reader is apt to agree with him when he realizes that this tribe of savages is actually a well-organized social group with its standards, morals, and taboos not so very different from those of our own more highly civilized society.

THE ECONOMICS OF PRIMITIVE PEOPLES.

*By Stephan Viljoen. P. S. King and Son, London.* 12s. 6d. 8½ x 5½; 282 + 2 plates; 1936.

This is a synthesis of the social conditions of primitive peoples with special reference to their ways of making a living. In order, the author discusses the influence of environmental conditions, climate and other geographical elements; then the demographic factors resulting from their mode of living and customs. There follow chapters on the several types of economy found among them, the variations observed among peoples following the same general type, the difference in the development of arts and crafts, division of labor, class division, trading technique, and money. In brief, it is a complete survey of the economic system as manifested among peoples as diverse as Eskimos and Bushmen. Moreover, while it is mainly descriptive, the author shows a remarkable ability in deftly disposing of theories that do not correspond to facts. There is an index and a fairly adequate bibliography.

RATS, PLAGUE, AND RELIGION. *Stories of Medical Mission Work in India.*

*By John Spencer Carmen. The Judson Press, Philadelphia.* $1.25. 7½ x 5; viii + 246 + 11 plates; 1936.

The author of this small volume gives in a series of realistic and intensely interesting incidents, some of his experiences as a medical missionary in India. As might be inferred from the title it is not altogether pleasant reading. Nevertheless the book fascinates, stirs sympathy, and leaves a lasting impression of the excellent work being done by some missionaries in foreign fields. Dr. Carmen describes some of the deplorable sanitary and hygienic conditions existing in rural India, and tells how they are being remedied even in the face of strong opposition because of customs, castes, and superstitions.

The book has several maps of the areas of India considered, and a number of photographs typifying life and living conditions in the country.

KÖRPERBAU UND CHARAKTER. *Untersuchungen zum Konstitutionsproblem und zur Lehre von den Temperamenten.*

*By Ernst Kretschmer. Julius Springer, Berlin.* RM. 13.60. 9¾ x 6½; x + 243; 1936.

This new edition of Kretschmer's epoch-making treatise on physique and temperament differs only slightly from the preceding ones. The exposition of the subject matter remains practically the same except for some additions. These include a summary of the principal studies which confirm his findings and a report of a few of the more recent investigations on the different physiological reactions observed in asthenics and pyknics. Of particular interest is the author's reply to those who have stated that the somatic types are really racial physical characteristics and that the temperaments associated with them are expressions of differences in racial psychology. Kretschmer presents data derived from an investigation on Japanese which show that the distribution of the three somatic types in the general population is markedly different from that found either in circular or schizophrenic individuals.

LEAVES FROM THE JUNGLE. *Life in a Gond Village.*

*By Verrier Elwin. John Murray, London.* 9s. net. 8½ x 5½; 243 + 9 plates; 1936.

In the forests of Central India lives an aboriginal tribe, the Gonds, to whom Verrier Elwin and his helpers have tried for several years to bring relief from physical suffering and, to their children, "some of the elements of hygiene and healthy living." This book is an entertaining account, in diary form, of the author's experiences over a period of four years and includes an introductory chapter on the Gonds, their ways of life, their philosophy, and their problem of adjusting themselves to the rest of the world.

The text is illustrated with photographs, and at the end of the book there is a section of notes elaborating on some of the things referred to in the diary.

THE PHYSICAL ANTHROPOLOGY OF THE SEMINOLE INDIANS OF OKLAHOMA.

*By Wilton M. Krogman. With an Introduction by Corrado Gini. Comitato Italiano per lo Studio dei Problemi della Popolazione, Rome.* Lire 20. 10 x 7; xi + 199 + 39 plates; 1935 (paper).

For one summer in the field, Dr. Krogman and his party collected an extraordinary amount of information and apparently did it very thoroughly. Their major purpose at the outset was to investigate the racial effects of Indian-Negro crossing. For this purpose, as much information as possible was gathered about the family history and relationship between the individuals studied. Particular attention was given to skin color. The author does not feel that his data are sufficient to demonstrate the physical effects of the cross between Negro and Indian. However, there is evidence in this study that a large part of the Indian blood in Negro veins has come from Seminole and Creek Indians.

OFFICIAL YEAR BOOK OF THE COMMONWEALTH OF AUSTRALIA. *Number 28, 1935. Prepared by E. T. McPhee. Commonwealth Bureau of Census and Statistics, Canberra.* 5s. 9 x 6; xxxi + 971; 1936.

This year book supplies data concerning the state of things in Australia in 1934, together with some historical material. Information is given on colonization, physiography, government, trade, education, public justice, public hygiene, vital statistics, labor, wages and prices, the utilization and preservation of natural resources, manufacturing and transport and communication. A chronological table of the chief events since the establishment of settlement in the Commonwealth, a list of the official statistical publications of Australia, a "select list of representative works dealing with Australia," and an index are provided.

DAS BEVÖLKERUNGSPROBLEM UND SEINE AUSWIRKUNG IN DER NEUEN DEUTSCHEN STEUERREFORM.

*By Karl Barth. Hans Buske Verlag,*

*Leipzig*. RM. 4.50. 9¼ x 6; 158; 1936 (paper).

This pamphlet gives a general account of the trends in the German population particularly in the last few years in respect to all the commonly noted variables: age and sex distribution, birth and death rates, emigration, etc. As might be expected, these statistics are fitted into a general discussion of population to prove both that Germany is overcrowded and needs more land and that it is the patriotic duty of every German to raise a large family.

ABRAHAM. *Recent Discoveries and Hebrew Origins*.

*By Sir Leonard Woolley. Charles Scribner's Sons, New York*. $3.00. 7⅞ x 5½; 299; 1936.

One of the most distinguished authorities on Mesopotamian archeology presents evidence that the biblical Abraham was an historical character, born in the metropolis Ur during the 20th Century B.C.—or, more probably, that he is now three personalities rolled into one. About a third of the book gives pure description of the ancient urban culture. The balance specializes on Abraham, although also rich in cultural data. The style is delightful. A short index and chronology is appended.

HIGH UP IN MEXICO.

*By O. A. Merritt-Hawkes. Ivor Nicholson and Watson, London*. 15s. net. 8¾ x 5⅝; xi + 279 + 17 illustrations; 1936.

A somewhat impressionistic picture of a wide-ranging variety of Mexican scenes and persons, brightly and smartly painted by an Englishwoman trained as a biologist. The book evidently has no very deep purpose beyond the laudable one of entertaining the reader. This it does. The intending visitor to Mexico will find it interesting and stimulating, particularly if he intends to concern himself at all with the sociological problems that present themselves under the present regime.

## ZOÖLOGY

MÉLANGES PAUL PELSENEER. *Mémoires du Musée Royal d'Histoire Naturelle de Belgique, Deuxième Série, Fasc. 3*.

*Musée Royal d'Histoire Naturelle de Belgique, Bruxelles*. 13 x 9¾; 1206; 1936 (paper).

This ponderous volume is a testimonial to one of the foremost living exponents of systematic zoology. Pelseneer is probably the last representative of that school of natural science typified by Lamarck in France, Swainson in England, and Rafinesque in the United States, within the scope of whose intellects was embraced the entire field of taxonomic zoology. By way of expressing their high esteem for his accomplishments, seventy-four of the world's leading biologists have contributed sixty-eight miscellaneous articles, drawn from the fields of zoology, botany, geology, genetics, and medicine.

The reviewer can mention only a few papers which seem of great interest to him personally in this miscellany of contributions. First among these is a *History of the Classification of the Mollusca*, by Aug. Lameere, of Brussels, which summarizes the opinions of all authorities of importance from Aristotle down to his own publication of a few years ago. Strangely enough, although this article is dedicated to Pelseneer by its author, no mention is made to Pelseneer's own contribution to molluscan taxonomy in Lankester's *Treatise on Zoology*, Vol. V. Neither does he mention the taxonomy of bivalves which Dall contributed to the last edition of Zittel's *Paleontology*. Lameere's most striking contribution to the classification of mollusca is the union of the so-called paleozoic pteropods discovered by Walcott in Canada, with the Bellerophonts.

Next in interest is probably the discussion of the Nudibranchiata by Nils Hj. Odhner of Stockholm, the longest article in the volume, and then one by Arnold Pictet of Geneva on the effect of geography

on genetics. The other contributions are equally meritorious but it is obviously impossible to do justice to all of them here.

STUDIES IN EXPERIMENTAL ZOOLOGY. (*Regeneration, Experimental Embryology, Endocrinology.*)

*By A. Elizabeth Adams. A. Elizabeth Adams, Mt. Holyoke College, South Hadley, Mass.* $1.25. 10¾ x 8½; v + 74; 1936.

LABORATORY STUDIES IN GENERAL ZOOLOGY.

*By Ann H. Morgan. Ann H. Morgan, Mt. Holyoke College, South Hadley, Mass.* $2.00. 10¾ x 8½; iii + 135; 1936.

It is a wonder that more laboratory manuals dealing with experimental phases of biology have not appeared. This is certainly an instructive and profitable field for the student as it not only teaches him technique, theory and scientific discipline but allows him to get a brief taste of scientific research as well. There is certainly no logical reason why college laboratories of zoölogy should ban living animals from their premises. In fact, it might be a good plan to show the student a live organism once in a while just for luck. The adoption of Doctor Adam's manual will not only require the presence of living creatures in the laboratory but will demand that the young zoölogist make certain manipulations with them. In this book the author has skillfully organized a group of classroom studies chosen from the fields of regeneration, experimental embryology and endocrinology. The following data are included under each individual exercise: (1) object of the experiment; (2) technique to be followed; (3) observations to be made and recorded, and (4) references to research investigations. As a result of original planning and ingenious treatment *Studies in Experimental Zoology* rises considerably above the run-of-mine manual and can be heartily recommended.

*Laboratory Studies* by Morgan, while more orthodox than the preceding book, is likewise a well-prepared guide for elementary students. The volume covers the phases of morphology, physiology and ecology commonly studied in college laboratories. Its chief attributes lie in a careful selection of material; an authoritative treatment and organization of that material; a clear style of presentation, and an excellent cross-reference system to other textbooks and papers. The reviewer feels the author has done all in her power to make the book as useful as possible for the student even to the extent of advising him to "be on time."

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society, Volume XXI, Numbers 12–18.*

*New York Zoological Society, Zoological Park, New York.* $1.75. 10½ x 7; 96 + 22 plates; 1936 (paper).

The following papers are included in this issue of *Zoologica:*

1. The Morphology, Cytology and Life-history of *Oodinium ocellatum* Brown, a Dinoflagellate Parasite on Marine Fishes. By Ross F. Nigrelli.

2. The Winter Movements of the Landlocked Alewife, *Pomolobus pseudoharengus* (Wilson). By C. M. Breder, Jr. and R. F. Nigrelli. This study was prompted by complaints to the New York City Water Department of fishes emerging from household taps. Odell(1934) wrote of this same species but had no data on the whereabouts of the fish during the winter months, whereas this is the period best covered by the data secured from the Department of Water Supply.

3. Systematic Notes on Bermudian and West Indian Tunas of the Genera *Parathunnus* and *Neothunnus*. By William Beebe and John Tee-Van. Plates are included with 13 figures of specimens of these two species.

4. Food of the Bermuda and West Indian Tunas of the Genera *Parathunnus* and *Neothunnus*. By William Beebe. Many surprises of great interest were uncovered during the course of examination of the stomach contents of these two genera. New and rare species of fish and invertebrates were found, and some forms generally supposed to be a common item of diet—such as copepods—were completely missing.

5. Notes on the Biology and Ecology

of Giant Tuna, *Thunnus thynnus* Linnaeus, Observed at Portland, Maine. By Jocelyn Crane. Descriptions are based on 34 newly-caught tunas.

6. The Templeton Crocker Expedition. I. Six New Brachyuran Crabs from the Gulf of California. By Steve A. Glassell.

7. Neoplastic Diseases in Small Tropical Fishes. By G. M. Smith, C. W. Coates and L. C. Strong. A tendency toward neoplasia has been noted in only 5 species out of an approximate 400 under observation at the New York Aquarium during the last five years. These diseases are described, and a group of pigmented cutaneous tumors in the hybrids of the Mexican killifish are discussed from the genetic viewpoint.

CONTRIBUTIONS TO A SCIENCE OF NEMATOLOGY. *Numbers I to XXVI.*

*By N. A. Cobb. (Obtainable from Mrs. F. C. Blanchard, Botanical Gardens, University of Michigan, Ann Arbor.)* $5.35. $9\frac{5}{8}$ x $6\frac{3}{4}$; 490; 1935.

Dr. N. A. Cobb was a remarkable man on many accounts. His was an adventurous, original and wide-ranging mind, in which orthodoxy (in the widest sense of the word and not merely the religious) was conspicuous by its absence. He was a non-conformist *pur sang*, and for that and other reasons a delightful person.

If any single part of his manifold interests could be separated from the rest and justly characterized as his life-work it would be his work over many years on nematodes, or, as he preferred to call them, "nemas." For more than a third of a century he ranked as one of the world's foremost authorities on this group. Long before the word "biometry" had come into general use, or any considerable amount of work in the modern sense had been done in that field, Dr. Cobb had invented and put into use a biometric scheme for the specific identification of nematodes. In its essentials it is today established as a foundation stone of nematode taxonomy.

This volume is Cobb's enduring monument as a nematologist. It consists of 25 papers of varying length which together at least touch upon where they do not exhaustively treat, virtually every aspect of nematode biology. Some of them are reprinted from journals in which they originally appeared. Others find their first publication in this volume. The whole expense of manufacturing the volume he and his family privately contributed at no small sacrifice, and with no other motivation than to contribute to the advancement of the science of nematology.

The volume constitutes a unique reference work. Zoological laboratories, libraries and individuals owe it as a duty to themselves, to their science, and to the memory of a great colleague to purchase and make more widely available this book.

THE TSETSE FLIES OF EAST AFRICA. *A First Study of Their Ecology, With a View to Their Control. Transactions of the Royal Entomological Society of London, Vol. 84.*

*By C. F. M. Swynnerton. With a Preface by Right Hon. W. Ormsby-Gore. Royal Entomological Society of London, London.* £5 10s. $10\frac{1}{4}$ x $6\frac{3}{4}$; xxxvi + 579 + 22 plates + 7 folding maps; 1936 (paper).

This is a complete report of the work done in the past few years by the Tsetse Research Department of Tanganyika together with their hopes and general plans for the future as well as an account of all that is known of this fly and its natural habitat. The effects of temperature, humidity and land conditions on their breeding, activity and migration is discussed in great detail. The main concern of the authors is to devise means of preventing the spread of the flies and reclaiming areas now occupied by them. At the same time they have to provide water and other necessities to make the land fit for settlers. A large part of the book is devoted to descriptions of various facts about Tanganyika territory, and in particular the need for larger appropriations from the government to carry on the good work is stressed. In such a practical, factual study we were pleasantly surprised to find a most interesting discussion of "man's right to destroy the

tsetse fly" in which this much maligned animal is viewed as a preserver of the natural beauty and resources of the country!

This is a contribution to the literature of ecology of the first rank of importance.

THE BIOLOGICAL CONTROL OF INSECTS. *With a Chapter on Weed Control.*

*By Harvey L. Sweetman. Foreword by L. O. Howard. Comstock Publishing Co., Ithaca.* $3.75. 9 x 6; xii + 461; 1936.

In essence, the problem of the control of one (or more) species by another is a population problem. It is only recently that the economic biologists, as a group, have appreciated and grasped this point. Stimulated and informed by the basic contributions of such workers as Volterra, Pearl and Lotka they are turning out regularly data that furnish both practical aid in the limitation of undesirable pests and contribute as well to the more theoretical aspects of biology generally.

The present book is a good example of this modern development. It presents, in a sane and well organized fashion, a wealth of practical material about the technique of insect control without losing sight of the underlying biological principles. The plan of the text is to consider by chapters the importance of various organisms in limiting the growth of injurious insect populations. Thus sections are devoted to microörganisms, parasitic lower invertebrates, insect parasites and predators and other forms that have value for the economic biologist. Each chapter attempts to outline and appraise the particular control method under discussion. Many studies are cited in the text; these are cross-referenced in the bibliography. The book is illustrated and contains an index and glossary.

Миграции Рыб.

П. Ю. Шмидт. Государственное издательство биологической и медичинской литературы, Москва-Ленинград. 4 р. 95 коп; 327; 1936.

[FISH MIGRATIONS.

*By P. J. Schmidt. State Publishing House for Biological and Medical Literature, Moscow-Leningrad.* 4.95 roubles. 5 x 8 inches; 327, 1936 (cloth)].

The study of fish migrations presents an attractive problem for biologists interested in evolutionary problems, primarily because the migration instinct and ways seem to be significantly conditioned not only by the present environmental situation, but also by those that have existed for millions of years past. But undoubtedly the chief reason for the variety and wealth of observations at present accumulated lies in their practical importance for the fishery industry. In the present volume Professor Schmidt treats the problem in detail, and discusses some important and hitherto unpublished data on the migration of herring, plaice and various salmons in the Far East, where he has been in charge of investigations for several years. There are five chapters: Migrations of pelagic, benthic, passing and freshwater fishes; and Environment and migrations. This is the book of an expert on the subject.

BIRD MIGRATION. *A Short Account.*

*By A. Landsborough Thomson. H. F. and G. Witherby, London.* 5s. net. $6\frac{7}{8}$ x $4\frac{1}{2}$; 224 + 6 plates; 1936.

Within the last twenty years great progress has been made in unravelling the riddle of bird migration. Studies have changed in character from cursory observations on bird movements to the systematic recording and mapping of migratory routes. In addition, the problem has recently been attacked experimentally with highly suggestive results. This general activity has accrued a wealth of technical data not always readily available to the naturalist and bird-lover. To remedy this situation Doctor Thomson has written the present book ". . . in response to a request for a short account of bird migration, in simple terms." The text is divided into four general sections and covers the historical, descriptive and dynamic aspects of the subject. The latter discussion is of great interest in that it attempts to picture the

relative rôle of ecological, physiological and psychological factors in controlling and initiating bird migration. It is becoming more and more obvious that only by a rounded study of all these factors can the true explanation of migration be reached.

This is a highly readable book. It has the attribute of simplicity without loss of precision and epitomizes a number of diverse facts. There is an annotated bibliography, an index and a number of text illustrations.

MODE OF LIFE, FEEDING, DIGESTION AND SYMBIOSIS WITH ZOOXANTHELLAE IN THE TRIDACNIDAE. *Great Barrier Reef Expedition 1928–1929 Scientific Reports, Volume I, Number 11.*

*By C. M. Yonge. British Museum (Natural History), London.* 5s. 12¼ x 9⅛; 39 + 5 plates; 1936 (paper).

The giant clams *Tridacnidae* may be divided into two groups according to the mode of life. One group contains surface living species, among which is the form that attains greatest size, *T. deraso*, 4½ ft. long; the other group contains the boring species *T. crocea*, and *T. fossor* that dig into coral boulders. One of the unique characteristics of this genus *Tridacna* is the unusual presence of the algae Zooxanthellae—in the tissues. The Tridacnidae in fact "farm" the algae in large quantities in their extremely enlarged mantles. The inner surface of the dorsal mantle edge contains lens like structures (hyaline organs) the purpose of which seems to be to effect internal illumination of the mantle tissues for the benefit of the Zooxanthellae, which are to be consumed in the course of time. *Tridacna* is probably the best example of the exploitation of associated algae by an animal. The Zooxanthellae are believed by the author to have caused the clams to evolve their gigantic size, since once established in the siphonal region it became advantageous to the *Tridacna* to extend the mantle tissues. This involved the twisting around of the mantle and displacement of the umbo and hinge, and the disappearance of other organs. The mantles were then free to develop enormously. A valuable contribution.

BIG GAME HUNTING IN MANCHURIA.

*By N. Baikov. Adapted from the Russian by Serge Ivanoff and Gertrude Mack. Hutchinson and Co., London.* 18s. net. 9 x 6; 285 + 17 plates; 1936.

A delightful series of sketches in which the author, a Russian trapper, describes his adventures in the Manchurian forests. We learn that big game hunting by no means provides the only thrills of the Manchurian wilds. Besides his adventures with Great Van, the tiger, and other forest animals some of the author's most thrilling and gruesome tales have been of the bloody deeds of the Khunghuz, the forest bandits who are more to be feared and more to be respected than all the wild life put together.

Blood and thunder and excitement, however, are far from the only recommendations for this entertaining volume. Simply and artistically the author revives for us the spirit and laws of the great Taiga and its many and varied inhabitants. The hunters, the trappers, the Khunghuzs, and the Varnaks who fled into the Russian wilds from Siberian imprisonment become in this enthralling volume living people with fascinating stories. Baikov writes too of forest kidnappings; vividly he describes the "Snakes' Grand-Dad" and this old hermit's mysterious powers over the deadly serpents. Ginseng seekers are realistically portrayed as they wander aimlessly and unarmed through the dangerous country in their religious and unending search for the famous ginseng—the Root of Life.

MINNOWS OF MICHIGAN. *Bulletin No. 8. By Carl L. Hubbs and Gerald P. Cooper. Cranbrook Institute of Science, Bloomfield Hills, Mich.* 50 cents. 9 x 6; 95 + 10 plates; 1936 (paper).

The authors emphasize the point that a true minnow is not just any small fish, but only those belonging to the family Cyprinidae. True minnows, and there are over 2000 species of them, are found

throughout the north temperate zone over North America, Europe and Asia. With the exception of one Japanese species they are only found in fresh water. Economically they are of great importance as they constitute a considerable part of the food for the important game fishes. The authors describe the ten most important and abundant varieties of the Michigan minnow giving detailed accounts of their breeding, spawning and feeding habits. The bulletin is illustrated by excellent photographs. It has an index and a good sized bibliography.

THE HABITAT AND FOOD OF THE CALIFORNIA SEA MUSSEL. *Bulletin of the Scripps Institution of Oceanography of the University of California, Technical Series, Volume 4, No. 1. Edited by Denis L. Fox. University of California Press, Berkeley.* 75 cents. 10¼ x 6¾; 64; 1936.

This work, by four coöperating authors, is a complete investigation into the life habits of a well known mollusk. Beginning with a discussion of the literature covering related forms, there follow accounts of original experiments on the degree of salinity tolerated by the California Mussel, the anatomy of its digestive tract, the nature of its food and the method of obtaining it, and chemical analysis of the digestive enzymes and of the faecal material. Not the least interesting feature of the investigation is that of the rate at which water is circulated through the siphons and over the gills. All told, this series of experiments is an excellent example of what a scientific investigation ought to be.

There is a table of contents, together with lists of tables and figures, but no general index.

RESULTS FROM BREEDING RABBITS THAT ARE SUCKLING YOUNG. *U. S. Department of Agriculture, Circular No. 410.*

*By Charles E. Kellogg. Government Printing Office, Washington.* 5 cents. 9 x 5¾; 8; 1936 (paper).

This study was undertaken to determine how intensively rabbits can be bred without detriment to the stock. While still suckling young from a previous litter—the young being weaned in this experiment at 56 days—one group of 40 does was mated 28 days after each kindling, a second group at 42 days, and a third group at 56 days. Records were kept as to number of young in litter weaned and average weight at weaning, service acceptance by the doe and the result of the mating. From the results of this experiment, of one year's duration, the author deems a 28 day breeding schedule inadvisable, a 42 day schedule as possibly advantageous during the period of the natural mating season, but in general the 56 day schedule as the best.

THE MAMMALS AND LIFE ZONES OF OREGON. *North American Fauna, No. 55. U. S. Department of Agriculture, Bureau of Biological Survey.*

*By Vernon Bailey. U. S. Government Printing Office, Washington.* 75 cents. 9¼ x 5¾; 416 + 52 plates + 1 folding map; 1936 (paper).

There has been a decided trend in recent years towards a greater interest in the conservation of the wild life of this country. This publication is another evidence of that trend. An attempt is made not only to list the species of this region, but to give information concerning their physical characters, distribution, abundance, habits and economic status, as a means for better understanding, appreciation, management and control of the fauna. The annotated list of species is preceded by brief descriptions of the physiographic features of the state and of the life zones, and by tables listing the mammals, reptiles, breeding birds and the plants of Oregon, by life zones. A bibliography, glossary of Indian names of mammals, and an index are included.

DEEP-SEA FISHES OF THE BERMUDA OCEANOGRAPHIC EXPEDITIONS. *No. 3—Family Serrivomeridae. Zoologica, Volume XX. Number 3.*

*By William Beebe and Jocelyn Crane. New York Zoological Society, New York.* 50 cents. 9½ x 6¼; 50; 1936 (paper).

This paper on the genus *Serrivomer* is based on a study of the type specimens in museums and the eels captured by the Bermuda Oceanographic Expeditions. There are three distinct species: *S. beanii* and *S. brevidentatus* from the Atlantic, and *S. sector* from the Pacific and Indian Oceans. The principal difference between the species are in dental and skeletal structures. Detailed descriptions are given of *S. beanii* and *S. brevidentatus*, including adult developmental growth stages, and of their ecology.

THE ORIGIN OF HIGHER CATEGORIES IN CYNIPS. *Indiana University Publications, Science Series No. 4.*

*By Alfred C. Kinsey. Indiana University Bookstore, Bloomington, Ind.* $2.50. 10 x 7; 334; 1936 (paper).

This is a systematic study of seventy species of gall wasps new to the genus *Cynips*. Most of the new species are from Mexico and this study includes all that is known of the Mexican and Guatemalan representatives of the group. New data have also been added on some of the species included in the author's 1930 monograph. "Higher categories" refer to sections inclusive of smaller sections (the lower categories) in a chain of species. Illustrated with pen and ink drawings and photographs, the monograph has an alphabetical index and complete bibliographies.

HOW TO KNOW THE INSECTS. *An Illustrated Key to the More Common Families of Insects, with Suggestions for Collecting, Mounting and Studying Them. Biological Survey Publication No. 1, Iowa Academy of Science.*

*By H. E. Jacques. H. E. Jacques, 709 N. Main St., Mt. Pleasant, Iowa.* $1.00. 8½ x 5½; 140; 1936 (paper).

The enthusiastic young entomologist should find this a most helpful guide in his attempt to build up a good insect collection. Clear directions for collecting and mounting insects are given and the key to the orders and principal families is made quite simple. Drawings illustrate the typical external structures which are used in classification, and there are about 200 pen and ink sketches of representative species. The guide has been prepared with special reference to the insects of Iowa but its usefulness is by no means limited to this region.

SUCHE, AUFNAHME UND ENZYMATISCHE SPALTUNG DER NAHRUNG DURCH DIE WELLHORNSCHNECKE BUCCINUM UNDATUM L. (*Grundlegung einer ganzheitlichen Deutung der Vorgänge im Beute- und Verdauungsfeld.*) *Zoologica, Heft 92.*

*By Friedrich Brock. E. Schweizerbart'sche Verlagsbuchhandlung, Stuttgart.* RM. 46. 12½ x 9¾; 136 + 1 plate; 1936 (paper).

The author discusses habits of feeding and problems of digestion in the marine snail *Buccinum undatum* L. There is a description of the anatomy of the snail, and the organs used for capturing and assimilating its food. The glands of the stomach and intestine were studied to determine what part they played in digestion. It was found that the glands of the upper intestine were not actual digestive glands though to some extent they break up connective tissue and fat. However neither olive oil, nor soluble starch nor saccharose were effected by these glands. This snail is a carrion eating animal.

LITTLE BLACK ANT.

*By Alice Gall and Fleming Crew. Oxford University Press, New York.* $1.50. 9 x 6½; 128; 1936.

This book describes life in an ant hill. It tells how the black ants secure food from the flowers and from their "honey cows"; of their wars with the red ants who seek to enslave them; and of their industry and coöperation.

It is written with a charming simplicity which should appeal to children and is delightfully illustrated.

DO YOU KNOW ABOUT FISHES?

*By Janet Smalley. William Morrow and Co., New York.* $1.25. 5¾ x 9; 45; 1936.

This little book for very small children is really good, in respect of both illustrations—which mainly fill it—and text. What it tells youngsters about a baker's dozen sorts of fish is sound, both in its implications as well as direct statements. Its zoology is vouched for by the Philadelphia Academy of Natural Sciences.

THE GENERA OF PARASITIC WASPS OF THE BRACONID SUBFAMILY EUPHORINAE, WITH A REVIEW OF THE NEARCTIC SPECIES. *U. S. Department of Agriculture Miscellaneous Publication No. 241.*

*By C. F. W. Muesebeck. U. S. Government Printing Office, Washington.* 5 cents. 9¼ x 5⅞; 38; 1936 (paper).

Detailed descriptions of the morphological characteristics of 14 genera are given as well as notes on geographical distribution, host-parasite relationships, etc. A key to the genera of Euphorinae is included. There is an index and a one page bibliography.

THREE NEW SPECIES OF FRIDERICIA (ENCHYTRAEIDAE) FROM CALIFORNIA. *University of California Publications in Zoology, Volume 41, Number 12.*

*By A. W. Bell. University of California Press, Berkeley.* 35 cents. 10¼ x 6¾; 20; 1936 (paper).

TRYPANOSOMA NEOTOMAE, SP. NOV., IN THE DUSKY-FOOTED WOOD RAT AND THE WOOD RAT FLEA. *University of California Publications in Zoology, Volume 41, Number 11.*

*By Fae Donat Wood. University of California Press, Berkeley.* 25 cents. 10¼ x 6¾; 12; 1936 (paper).

CALIFORNIA ISOPODS OF THE GENUS PORCELLIO WITH DESCRIPTIONS OF A NEW SPECIES AND A NEW SUBSPECIES. *University of California Publications in Zoology, Vol. 41, No. 13.*

*By Milton A. Miller. University of California Press, Berkeley.* 25 cents. 10¼ x 6¾; 8; 1936 (paper).

A KEY TO THE RATTLESNAKES WITH SUMMARY OF CHARACTERISTICS. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 20.*

*By Laurence M. Klauber. Society of Natural History, San Diego, Calif.* 10⅛ x 6⅞; 92 + 1 folding table; 1936 (paper).

NEW PORCELLANIDS AND PINNOTHERIDS FROM TROPICAL NORTH AMERICAN WATERS. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 21.*

*By Steve A. Glassell. Society of Natural History, San Diego, Calif.* 10⅛ x 6⅞; 28; 1936 (paper).

WEST AMERICAN SPECIES OF THE GENUS PHOS. *Transaction of the San Diego Society of Natural History, Vol. 8, No. 22.*

*By A. M. Strong and H. N. Lowe. Society of Natural History, San Diego, Calif.* 10⅛ x 6⅞; 16; 1936 (paper).

## BOTANY

OUR FRIENDS THE TREES.

*By P. G. Cross. E. P. Dutton and Co., New York.* $5.00. 9¼ x 6¼; 334 + 31 illustrations; 1936.

Here is a book about trees by a man who loves them and wishes others to enjoy with him some of the happiness which they have brought him. It is essentially a practical book, with much information as to the best kind of tree for different situations, as to how to transplant different kinds of trees, and how to care for them afterwards. All of this is most helpful to any one who has just built a home and is about to plant the grounds, but probably the reader who takes seriously the recommendation of the Ben Davis apple for eating will be disappointed.

The author displays a great deal of righteous indignation against the wanton slaughter of our forest trees, by those whose brains are in their pocket books. In this he seems to be amply justified. The lumber industry is of course a legitimate one, but it needs intelligent supervision before it is too late. Already the tallest Douglas fir in Canada, and the

tallest eucalyptus in Australia have fallen victims to the bandsaw in the name of progress—a fate which the coast redwoods have only narrowly escaped. Not only does the destruction of trees such as these constitute a calamity from the aesthetic standpoint, but the unsupervised deforestation of hill and valley opens the way for the rainfall to erode surface soil with annual floods, and so become a curse instead of a blessing.

This is not strictly a scientific book—note the use of the popular terms Douglas fir for a spruce tree (which is not a fir) and red cedar for a juniper (which is not a cedar) but in general it is scientifically accurate. All kinds of trees are discussed—those which bear fruit, those which bear nuts, those which are evergreen, and those whose foliage turns orange, purple, and scarlet in the fall. There is even a chapter devoted to the palms. In fact, the only tree whose absence makes it conspicuous is the gingko. This is surprising since it is a fairly rapidly growing tree, and harbors no parasites, which makes it ideal for anyone who wants trees about his home but who cannot devote all his time to caring for them.

The index covers eight pages. The illustrations are good, and each chapter is headed by a poem, the inevitable one of Joyce Kilmer being among them.

Теоретические Основы Яровизации. Т. Д. Лысенко. Государственное издательство колхозной и совхозной литературы, Москва-Ленинград. 1р. 95 коп; 152; 1935.

[Theoretical Basis of Vernalization. *By T. D. Lysenko. State Publishing House for Agricultural Literature, Moscow–Leningrad.* 1.95 roubles; 152; 1935 (cloth)].

In this excellent and important book Professor Lysenko describes his discovery and extensive investigations on the phenomenon of vernalization, namely, the acceleration of ear formation in winter varieties of cereals and in various other plants by exposing the germinating seeds to low temperature. The theoretical conception lying at the bottom of these investigations is that the processes of differentiation are essentially independent of growth, and that there are in differentiation definite stages or phases requiring particular sets of environmental conditions. Ear formation in winter varieties of cereals is impossible if a specific 'vernalization phase' proceeding under low temperatures is not completed. Artificially prolonged exposure of germinating seeds to cold in the spring can substitute for the natural course of events in the winter-fields. An outstanding feature of this line of investigation consists undoubtedly in the fact that laboratory experiments were very soon followed by extensive agricultural use of this valuable method for selection and various other practical purposes of plant breeding.

Coffee. *The Epic of a Commodity. By Heinrich E. Jacob. Translated by Eden and Cedar Paul. The Viking Press, New York.* $3.50. 9¼ x 6¼; xiii + 296 + 38 plates; 1935.

This book tells the history of coffee and its social and economic effects on civilization. The legend of its discovery in Arabia and its subsequent spread among the Mohammedans is given. "Coffee has sometimes been spoken of as the 'wine of Islam'; and, in actual fact, Mohammedan civilization, the Moslem love for drawing fine distinctions, for hair-splitting, for disputation—all the 'cold heat and flaming sobriety' of Arabic civilization, are closely connected with the effect of coffee upon the human brain." In contrast to this is the Bacchic culture of the classical period and of Christianity. The discussion of the psychological antithesis between coffee and wine as typifying different civilizations is extremely well done.

From the seventeenth century on, the use of coffee spread rapidly. It was brought into Europe by wars and commerce and moved on to other parts of the world during the period of the great colonial expansion. It affected the social and political life of most of western Europe through the establishment of coffee houses. And it partly took the place of beer which had been consumed in enormous quantities in England and Germany

in the Middle Ages. Finally the part Brazil plays as the producer of over ninety per cent of the world's supply of coffee today is discussed.

This book is extremely well written giving a sketch of world history for the past three centuries while telling the story of the production and consumption of coffee. As is often the case the author has perhaps become over enthusiastic on his subject. He has a brilliant imagination and his interpretation of the changes in the social order due solely to coffee is doubtless exaggerated but always interesting. There are many beautiful illustrations, a bibliography, and an index.

THE WESTERN RANGE. *Letter from the Secretary of Agriculture Transmitting in Response to Senate Resolution No. 289 a Report on the Western Range—a Great but Neglected Natural Resource. Senate Document No. 199, 74th Congress, 2d Session.*

*U. S. Government Printing Office, Washington.* 9¼ x 5¾; xvi + 620; 1936 (paper).

It is hoped that this treatise will aid in arousing public interest in the problem of conservation of the resources of the large western range which comprises about 40 per cent of the total land area of the continental United States. Depletion of the range appears to have been rapid, largely due to excessive stocking and consequent over-grazing. Erosion of the land due to misguided agricultural projects is another major source of depletion. It is estimated that not more than 5 per cent of the entire range area is now in a thoroughly satisfactory condition. The object of this publication has been to present "an all-inclusive survey of the range resource, its original and present condition, the causes and effects of changes, the social and economic function which it does and should render to the West and to the Nation, and, finally, to outline practical solutions for at least the more important problems." The field has been well covered in this excellent report. An index and extensive bibliography are included.

FERNS OF NORTHEASTERN UNITED STATES. *Illustrations and Descriptions of All Known Species in the New England and Middle Atlantic States. A Pocket Manual for the Amateur with a New Idea in Aids for Fern Identification.*

*By Farida A. Wiley. The American Museum of Natural History, New York.* $1.00. 3¾ x 6¼; 98; 1936.

This pocket manual contains drawings and descriptions of over 200 ferns. The lower pinnae of all the larger ferns are shown in life size, while the drawings of the entire fronds of large forms have been necessarily reduced. The scale of reduction is indicated in feet on the margin, or noted in the text. The small fronds are shown in life size. Except for four of the rare ferns, the drawings have all been made from specimens collected by the author and include all the known species found in New England and the middle Atlantic states. By the novel, simple, and clear methods developed in this work even the inexperienced should easily be able to identify these ferns. The manual contains an alphabetical glossary and index. There is no bibliography but the author gives credit in her foreword to the few references she has used.

THE COMPOSITION AND DYNAMICS OF A BEECH-MAPLE CLIMAX COMMUNITY. *Scientific Publications of the Cleveland Museum of Natural History, Volume VI.*

*By Arthur B. Williams. Cleveland Museum of Natural History, Cleveland.* 50 cents. 9¼ x 6¼; 92; 1936 (paper).

The modern student of ecology is convinced that it is practically impossible to make a proper community analysis unless both plants and animals are taken into consideration. Despite this growing opinion it is unusual to find a study that emphasizes the complete biota of any particular community. The present report is a step in this direction for the author has taken a 65 acre tract of beech-maple-hemlock forest and made careful surveys of its animal and plant populations as well as measurements of common environmental variables. In some of the

surveys the organisms have been individually counted and this quantitative emphasis is an important feature of the book. A number of interesting facts are reported. These are concerned largely with (1) the description of the interrelations existing between specific plants and animals of the community, and (2) the evaluation of climatic factors in shaping the growth of the community as a whole. The book has a moderately extensive bibliography and contains a number of graphs.

THE VEGETABLE GARDENER'S HOW BOOK. *By Chesla C. Sherlock. The Macmillan Co., New York.* $3.00. 8½ x 5½; xix + 285 + 28 plates; 1936.

This book contains fifty-seven chapters on gardening, each beginning with appropriate quotations by famous authors from St. Luke down to Sherlock's own abstracts from the *Ladies Home Journal*. It completely covers vegetable gardening, fruit and small fruit culture. The author gives simple and practical information on how to plant, culture, fertilize and harvest twenty-seven cooking vegetables, fifteen varieties for the salad garden, and fifteen kinds of fruit both for the orchard and the berry patch. Numerous photographs illustrate this complete volume and an appendix includes fertilizer, pruning, and spraying charts.

TEMPERATURE STUDIES OF SOME TOMATO PATHOGENS. *United States Department of Agriculture, Technical Bulletin No. 520. By Alice A. Nightingale and G. B. Ramsey. U. S. Government Printing Office, Washington.* 5 cents. 9 x 5¾; 36; 1936 (paper).

Data are presented in this paper to show the influences of temperature and maturity of fruit upon the growth and development of some fungi pathogenic to tomatoes. Nine organisms were selected for study, and records kept of the growth made on dextrose-potato agar plates held at 5° intervals of temperature between 30° and 95°F., and on tomatoes of varying degrees of ripeness at temperatures ranging between 32° and 85°F. Temperature, the pH of the agar and ripeness of the fruit were all found to be important factors in influencing the development of the pathogens.

IDENTIFICATION, HISTORY, AND DISTRIBUTION OF COMMON SORGHUM VARIETIES. *U. S. Department of Agriculture, Technical Bulletin No. 506. By H. N. Vinall, J. C. Stephens and J. H. Martin. Government Printing Office, Washington.* 25 cents. 9¼ x 5¾; 102 + 59 plates; 1936 (paper).

This report was prepared for the purpose of clearing away the confusion surrounding the identification of the many varieties of sorghum. This has been accomplished by giving (1) a description of the commercial varieties, (2) a key to assist in the identification of the varieties, (3) the estimated acreage and distribution of the important varieties, and (4) the known history of all varieties described. There are also included full page reproductions to scale of panicle, spikelets and kernels of about 60 recognized varieties. There is a bibliography.

BOTANISCHES VADEMECUM FÜR BILDENDE KÜNSTLER UND KUNSTGEWERBLER. *By G. Haberlandt. Gustav Fischer, Jena.* Rmk. 3.50 (paper); Rmk. 4.50 (cloth). 9¾ x 6½; viii + 80; 1936.

This little pamphlet is intended as a guide for artists, both professional and commercial, who need, according to the author, to know something of the anatomy of plants. Too many bad pictures have been drawn of flowers and there is a great need for artists to know something about their anatomy just as they must know something of the anatomy of the human figure. The author discusses the anatomy of the thallophytes, mosses and ferns, and the flowering plants. There are many excellent illustrations. This book will also be of use as a guide to the amateur botanist.

THE TROPICAL GARDEN. *Its Design, Horticulture and Plant Materials.*

*By Loraine E. Kuck and Richard C. Tongg. The Macmillan Co., New York.* $3.00. 8½ x 5½; x + 378 + 16 plates; 1936.

To those interested in gardens and gardening in the tropics this book is of the utmost value. It not only gives thorough information and practical advice concerning the plants suitable for tropical gardens, but also has chapters on design, plant culture, greenhouses, and horticulture in general. Color and blooming charts are included.

TREES AND SHRUBS OF KENYA COLONY. *A Revision and Enlargement of a Descriptive Catalogue of Some of the Common Trees and Woody Plants of Kenya Colony.*

*By E. Battiscombe. The Government Printer, Nairobi.* 5s. 9¾ x 6¼; xi + 201; 1936.

This book is purely a descriptive catalogue of the common trees and woody plants of Kenya. The families follow the classification system proposed by Hutchinson in his book *The Families of Flowering Plants*. The main part of the book, pages 1–173, comprises the descriptive list of the various species of plants. There are included an index of botanical names, index of vernacular names, and an index of English names.

SILVA FENNICA 37. *Lehdoista ja Lehtokasvien Leviämisestä Pohjois-Pirkkalan-Tyrvään Alueella.* [*Über Haine und Verbreitung der Hainpflanzen im Gebiet von Nord-Pirkkala-Tryvää.*]

*By Taimi Mäkelä. Society of Forestry in Suomi, Helsinki.* 9⅝ x 6½; 61 + 1 plate; 1936 (paper).

SILVA FENNICA 38. *Hankikylvöt Tuomarniemen Hoitoalueessa VV. 1913–1930.* [*Die 1913–1930 ausgeführten Schneesaaten im Rivier Tuomarniemi.*]

*By L. E. T. Borg. Society of Forestry in Suomi, Helsinki.* 9⅝ x 6½; 136; 1936 (paper).

ACTA FORESTALIA FENNICA 42. *Publications of the Society of Forestry in Suomi. Helsinki.* 9⅝ x 6½; vii + 589 + 5 plates; 1936 (paper).

## MORPHOLOGY

VERGLEICHENDE ENTWICKLUNGSGESCHICHTE DER TIERE. *In two Volumes.*

*By Korschelt and Heider. Revised by E. Korschelt. Gustav Fischer, Jena.* Two volumes Rmk. 52 (paper); Rmk. 56 (cloth). 10¼ x 7 inches; xx + 1314; 1936.

The first edition of this classic appeared nearly half a century ago, at a time when the science of invertebrate embryology was so young that it was possible to cover all of it in a book no bulkier than the present work. Since then the increase in knowledge of this subject has been so great that a modern edition constructed on the same general plan as the original would require at least half a dozen volumes, and many more than two collaborating authors. The reviser has had to select his material, and in doing so he has exercised the same good judgment as those familiar with the earlier editions have a right to expect from him.

The new edition is characterized by the same admirable illustrations, the same exhaustive bibliographies and references, together with many new ones, the same lucid exposition of embryonic phenomena augmented by summaries of and conclusions drawn from the works of all investigators of importance in this field in the twentieth century. Also the number of groups of uncertain phylogenetic position has increased, indicating perhaps that we do not know as much about evolution as we used to think we did. An innovation is the addition of a chapter devoted to the vertebrates, previous editions having been confined exclusively to invertebrate embryology.

The comprehensive index covers seventeen pages. The typography is excellent but the binding is very poor. Anyone contemplating the purchase of this book would do well to obtain an unbound copy.

BAILEY'S TEXT-BOOK OF HISTOLOGY (ELWYN AND STRONG). *Ninth Edition. Revised and rewritten by P. E. Smith, R. L. Carpenter, C. M. Goss, W. M. Copenhaver and A. E. Severinghaus. William Wood, Baltimore.* $6.00. 9 x 5¾; xvi + 773; 1936.

In revising the ninth edition of this well-known textbook the emphasis has been placed on dropping material out of date; adding new data; expanding passages to make them more inclusive, and generally clarifying the exposition. Certain figures have been discarded and many new illustrations added. These various revisions have been undertaken, as a coöperative enterprise, by five members of the Anatomy Department of Columbia University who have attempted ". . . to present, fairly, the major controversial differences of opinion" and to exclude ". . . a considerable body of facts which should rightly have been included if this were a source book for teachers and research workers rather than a textbook for students." The volume is logically organized for classroom use and possesses a good index. The figures are of first-rate calibre.

CYTOLOGIE DU LIQUIDE CÉPHALO-RACHIDIEN NORMAL CHEZ L'HOMME. *Monographie Critique et Pratique. By H. Jessen. Masson et Cie, Paris.* 40 francs. 10 x 6½; 168; 1936 (paper).

Although the cerebro-spinal fluid has been the subject of much study little definite and conclusive information has been obtained concerning the normal morphology and number of the cells. This book is devoted to a critical exposition of the various methods which have been employed by others, and a detailed presentation of the author's own researches on the subject. The author formally disclaims any intention of presenting his results as conclusive, or as serving as anything more than a point of orientation. Nevertheless, this is an important contribution. A bibliography covers twenty pages.

DAS MUSKELSPIEL DES MENSCHEN. *By Hermann Hoepke. Gustav Fischer, Jena.* Rmk. 5.50. 9⅛ x 6½; viii + 83; 1936 (paper).

An elementary description of man's muscular system and its functions, with a minimum of references to bones and practically none to other organs, is here presented. It is not a textbook or guide, but would be of value to non-laboratory students. The fifty illustrations are mostly excellent diagrams of selected muscles on a skeletal background to show origins, insertions, general positions and functions; the balance are photographs of nudes straining certain muscles for special demonstration.

## PHYSIOLOGY AND PATHOLOGY

THE PROBLEM OF NUTRITION. *Volume I. Interim Report of the Mixed Committee on the Problem of Nutrition. Series of League of Nations Publications II. Economic and Financial 1936. II. B. 3. League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York.* 50 cents. 9½ x 6¼; 98; 1936 (paper).

THE PROBLEM OF NUTRITION. *Volume II. Report on the Physiological Bases of Nutrition drawn up by the Technical Commission of the Health Committees at the meeting held in London (November 25th–29th, 1935), revised and amplified at the meeting held at Geneva (June 4th–8th, 1936). Series of League of Nations Publications II. Economic and Financial 1936. II. B. 4. League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York.* 15 cents. 9½ x 6¼; 27; 1936 (paper).

THE PROBLEM OF NUTRITION. *Volume III. Nutrition in Various Countries. Series of League of Nations Publications II. Economic and Financial 1936. II. B. 5. League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York.* $1.40. 9¼ x 6¼; 271; 1936 (paper).

THE PROBLEM OF NUTRITION. *Volume IV. Statistics of Food Production, Consumption and Prices. Documentation Prepared by the International Institute of Agriculture. Presented*

*to the Mixed Committee on the Problem of Nutrition at Its Second Session, June 4th, 1936. Series of League of Nations Publications II. Economic and Financial 1936. II. B. 6.*

*League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York.* 75 cents. 10½ x 8; 110; 1936 (paper).

In 1935 the Health Organization of the League of Nations set up a Mixed Committee on the Problem of Nutrition to make a detailed investigation into the agricultural, economic and health aspects of nutrition.

Vol. I in the series of reports presented by this committee is essentially an analysis of the problem, and an outline of the principles of nutrition and their application through which it is to be hoped national health may be improved. Volume II gives data on the average food requirements (calory, protein, fat, mineral and vitamin) for males and females of different age groups, and for special cases such as heavy labor and pregnancy. Volume III presents a survey of the measures taken in about 20 different countries to bring about an improvement in the nutrition in the various sections of the population. The following chapters are included under this heading: (1) Measures taken on behalf of mothers and infants; (2) measures taken on behalf of children of school age and young people; (3) measures taken on behalf of adults with special reference to unemployed adults, (4) army and navy dietaries, (5) measures to enable particular categories of consumers to obtain foodstuffs at reduced prices, (6) measures for ensuring the quality of foodstuffs, (7) research, education and popular instruction. Volume IV is submitted as a provisional document. Statistics on production, consumption, and prices of the protective and other foodstuffs are summarized in tables for each of about 20 different countries. The statistics on production and consumption are largely estimates and are accompanied by comments as to their scope and significance.

THE PLACE OF PSYCHOLOGY IN THE MEDICAL CURRICULUM *and other Papers. Individual Psychology Medical Pamphlets No. 16.*

*By Sir Walter Langdon-Brown, R. G. Macdonald Ladell, Frank Gray and F. G. Crookshank. C. W. Daniel Co., London.* 2s. 6d. net. 8½ x 5½; 56; 1936 (paper).

The four papers included in this volume have been written by doctors who believe that psychology is essential in the practise of medicine. Sir Walter Langdon-Brown, in his paper on the place of psychology in the medical curriculum, stresses this point and advocates some education in this field for medical students. Dr. Ladell, in a paper entitled medical psychology: pre-war, war-time and post-war, stresses the importance of psychology in diagnosing and treating diseases. Citing instances from his own career, he shows how post-war methods are much more accurate and effective than were pre-war methods and how the war was instrumental in awakening doctors to the reality of functional diseases. He concludes that since no distinct line can be drawn between functional and organic diseases, the medical man as well as the psychiatrist and the psycho-analyst should have some understanding of psychological methods. Dr. Gray discusses the psycho-pathology of organic disease and after citing a number of cases arrives at the rather surprising conclusion that "every illness is the neurotic solution of an internal conflict." Dr. Crookshank's paper deals with Adler's theory of organ inferiorities. It discusses the effects of organ inferiorities on the development of character, personality, and diseases (functional and organic) and concludes, very much as Dr. Gray does, that "the way to right living and avoidance of disease . . . is in the control of the psychical life."

MOTOR PERFORMANCE OF THE DEAF. *Comparative Psychology Monographs, Vol. 13, No. 6, Serial No. 66.*

*By Joseph E. Morsh. Johns Hopkins Press, Baltimore.* $1.00. 10 x 6¾; 51; 1936 (paper).

About 150 children with auditory defects ranging from 25 per cent to complete hearing loss were given tests of ability in motor performance. These included a tapping test with the Dunlap double tapping plate, a static hand control or steadiness test with Dunlap's apparatus for the purpose, a test for visual memory and location memory devised by the author, and Johnson's speed of eye movement and hand-eye coördination tests. Except for this last test, the performance of the subjects with impaired hearing was compared to that of an approximately equal number of children with good hearing. The results appear to indicate that the hard of hearing children demonstrate greater imbalance in the double-plate tapping test but their average speed is about equal to that of the other children. They are superior to the normal hearing children in the steadiness test, except when blind-folded, and in the location memory test, although for this they tend to confuse the associated objects more frequently than do the control children. The latter are superior in the speed of eye-movement test. It is not clear how significant are these results and consequently what conclusions may be drawn from them. The data are presented in a manner deserving strong criticism. There is no index or bibliography.

THEORETISCHE GRUNDLAGEN ZUM AUFBAU EINER BIOLOGISCHEN MEDIZIN.

*By Karl Kötschau and Adolf Meyer. Theodor Steinkopff, Dresden and Leipzig.* RM. 13 (cloth); RM. 12 (paper). 9 x 6; xvi + 217; 1936.

This book represents an attempt to present the various philosophical concepts derived from biology in a manner useful and stimulating to the medical profession. The first section deals with such subjects as mechanism and vitalism, relation of body to "soul," the holistic conception of psychology, and the "Gestalt" theory. Section II more fully covers the holistic concepts of theoretical biology and psychology. Section III deals with parallel problems of medicine and biology. After an introduction on the common origin of biological and medical thought, the authors devote the rest of this part to a critical discussion of the attitude in medical scientific thought that seeks to reduce everything to a physical-chemical basis. This involves a discussion of the rôle of mathematics and statistics in medicine. Section IV, entitled *Laws and Rules*, is a philosophical discussion of induction and deduction in relation to medical thought and techniques.

This is an important contribution to the literature of theoretical biology and medicine.

LABORATORY PRACTICE. *Manual of Public Health.*

*By J. R. Currie and Contributors. William Wood and Co., Baltimore.* $6.75. 8½ x 5½; xix + 378; 1936.

This book is designed to aid public health officers cope with the practical and technical problems that arise during the routine of their duties. It is a laboratory guide in the sense that it gives directions for the study and appraisal of a host of unrelated topics and a text in the sense that a number of these topics are discussed briefly in terms of their theoretical background. The book is divided into the following six sections: chemistry, bacteriology, protozoölogy, helminthology, entomology and meteorology. These diverse sections are related only to the extent that they contain material of conceivable importance for the sanitarian. Thus, in the chapter on bacteriology, a discussion of the staining technique for gonorrhoea films is found, while in the meteorology chapter the physical nature of fog is explained.

It is difficult to predict what real usefulness this volume will have. In matters of fact and organization the treatment appears excellent. In our opinion, however, the general scope is too heterogeneous to warrant much serious usage. There is an index and the text contains 169 adequate illustrations.

LIVE LONG AND BE HAPPY. *How to Prolong Your Life and Enjoy It.*

*By Lewellys F. Barker. D. Appleton-Century Co., New York.* $2.00. 7½ x 5; viii + 224; 1936.

As the medical profession makes its strides toward prolonging the life of mankind in general, the average person naturally wants to know what he can do to increase his own life span and at the same time be free from suffering. To the person of average intelligence many medical terms and explanations are beyond comprehension. There has been a definite need for a book that would explain in language readily understandable to the layman the latest developments in medicine designed to prevent disease and prolong life. Dr. Barker's book fills just this need. Written in simple, every day language it tells the layman not only what aids medical science offers for prolonging life, but also how to make the most of added years. There is a brief index.

VERGLEICHEND- PHYSIOLOGISCHES PRAKTIKUM. *Mit Besonderer Berücksichtigung der Niederen Tiere.*

*By W. v. Buddenbrock and G. v. Studnitz. Julius Springer, Berlin.* RM. 9.60. 9½ x 6; vi + 127; 1936.

A new manual encouraging a greater use of invertebrates for experimental and demonstration purposes. The authors believe that invertebrates serve the purpose just as well as higher animals in some cases in the demonstration and study of physiological phenomena, cost less, and require simpler and less expensive apparatus. Some marine forms are included in experiments outlined here. The experiments are arranged under the headings of the various senses, including chemical and kinetic; blood; water economy; nutrition and excretion. Many are suitable for high-school courses. American teachers can get some useful hints from a perusal of this book.

ABSORPTION FROM THE INTESTINE.

*By F. Verzár, assisted by E. J. McDougall. Longsmans, Green and Co., New York.* $9.00. 8¾ x 5⅞; xii + 294 + 12 plates; 1936.

The authors give a review of the more important experimental work, particularly recent work, dealing with the absorption of water, inorganic, and organic substances by various portions of the intestines, and the rôle this plays in regulating hydrostatic pressure. The discussion centers about their own research, which has mainly been to determine whether or not the physical laws of diffusion through a semipermeable membrane are sufficient to account for the activities of the intestine without appealing to biological selective forces. A very extensive bibliography is included.

NUTRITIONAL FACTORS IN DISEASE.

*By William R. Fearon. William Heinemann (Medical Books), London.* 7s. 6d. net. 8½ x 5¼; xiv + 141; 1936.

The substance of this book is based on the author's essay which won the 1935 Buckston-Browne prize given by the Harveian Society of London. In the first part of the book are listed the chemical elements which enter into the composition of the human organism or occur in the usual human diet. The caloric consumption of man is also given a brief discussion. The second part includes chapters on the relation of disease to alterations of carbohydrate, protein and lipid balance. The inorganic elements and inorganic and organic microconstituents of foodstuffs are described in the three remaining sections. The exposition of the subject matter is unusually lucid and objective. There is an index and a bibliography of 268 titles which attest to the scholarly quality of this work.

RAPPORT SUR LE PÈLERINAGE AU HEDJAZ *de l'Année de l'Hégire 1354 (A.D. 1936). Conseil Sanitaire Maritime et Quarantenaire d'Egypte, Alexandrie.* Free. 12½ x 9¼; 126 + 7 plates + 5 folding tables; 1936 (paper).

Each year there are a great number of people who make the pilgrimage to

Hedjaz, the vilayet in Arabia that has Mekka as its capital. And each year this report of the health conditions is published. It includes data on the number of pilgrims, the countries from which they come, the health regulations of those countries, the results of medical and bacteriological examinations at quarantine ports, and the number of caravan ships passing through the Suez Canal. It is interesting to note that while the number of pilgrims has steadily increased, the health conditions, due to better sanitary conditions and strict public health regulations, have steadily improved.

MEDICAL CLASSICS. *Volume 1, Number 1. Compiled by Emerson Crosby Kelly. Williams & Wilkins Co., Baltimore.* Subscription price $10.00 per volume; single copies $1.25. 10 x 7; 78; 1936 (paper).

*Medical Classics* aims to present to the medical man in their original forms some of the classical discoveries by famous physicians. The first number opens with the works of Sir James Paget. There is a short biography and a complete bibliography, followed by two of his most famous articles, one on *Osteitis deformans* and the other on Paget's disease of the nipple. These papers are both given in full in the belief that as nothing more is known now about either of these diseases than Paget himself knew, the medical man of today will be better able to deal with the subjects for having read them in their entirety.

EPIDEMIC AMEBIC DYSENTERY. *The Chicago Outbreak in 1933. National Institute of Health Bulletin No. 166.*

*U. S. Treasury Department, Public Health Service. Government Printing Office, Washington.* 20 cents. 9¼ x 5¾; xi + 187 + 2 plates + 3 folding charts; 1936 (paper).

This is exclusively an account of the Chicago epidemic of 1933 which originated in two hotels. Apparently certain employees were infected and the disease was spread from them due mainly to a sewerage leakage into the drinking water. The book is divided into three separate studies (1) the epidemiology of the outbreak, (2) sanitary engineering in the hotels concerned, and (3) a clinical study of thirty-five cases.

LES HYPERGLYCÉMIES. *Étude Clinique et Physiopathologique.*

*By Henri Warembourg. Preface by Professors Loeper and Polonovski. Masson et Cie, Paris.* 65 francs. 10 x 6½; xiii + 584; 1936 (paper).

Part I of this book contains an analysis of the current notions and facts relating to the general problems of glycemia. In Part II the author presents in detail the hyperglycemic reactions resulting from a series of physiological and experimental conditions. Parts III and IV discuss the diabetic and non-diabetic hyperglycemias, respectively, and the treatments tried for the maintenance of sugar regulation during such conditions. Much original work is included. The chief interest of the book will be to clinicians and pharmacologists. The bibliography covers 55 pages printed in small type. A monograph of first rate importance.

L'OSTÉOSYNTHÈSE AU POINT DE VUE BIOLOGIQUE. *Influence de la Nature du Métal (Étude Expérimentale).*

*By G. Menegaux and D. Odiette. Preface by J. Verne. Masson et Cie, Paris.* 37 francs. 10 x 6½; vii + 174; 1936 (paper).

The authors experimented with various simple metals and alloys in an attempt to find the most suitable and practical from the points of view of non-toxicity, cost, availability, and malleability for osteosynthetic purposes. The experiments described in detail in this book were made with cultures of bone tissue and with live animals. The three rustless steels, V² A Extra, Nicral D, Platino-stainless D, were the only metals tested which met all the requirements. There is a bibliography of five pages but no index.

TOXICITY OF FOOD CONTAINING SELENIUM AS SHOWN BY ITS EFFECT ON THE RAT. *U. S. Department of Agriculture. Technical Bulletin No. 534.*

*By Hazel E. Munsell, Grace M. DeVaney and Mary H. Kennedy. Government Printing Office, Washington.* 5 cents. 9⅛ x 5⅞; 26; 1936 (paper).

Wheat containing selenium when fed to rats produced toxic symptoms; in particular generalized edema and liver injury. The lethal dose, the detrimental effect on growth and reproduction, the storage of selenium in the body of the rat, and the after effects of selenium are discussed.

QUARTERLY BULLETIN OF THE HEALTH ORGANISATION. *Volume V, No. 3.*

*Health Organisation, League of Nations, Geneva; World Peace Foundation, 8 West 40th St., New York.* Annual subscription $2.50; separate issues 65 cents. 9½ x 6⅛; 180; 1936.

## BIOCHEMISTRY

IL GLUTATIONE. *Sua importanza nella biologia generale e nella fisiopatologia umana.*

*By Giuseppe Barbaro-Forleo. Preface by Luigi Zoja. Tipografia già cooperativa, Pavia.* 30 Lire. 10 x 7; xiv + 449; 1936 (paper).

Since its discovery, glutathione has been the object of numerous investigations and much speculation regarding its action. The staggering amount of literature thus accumulated is here critically and thoroughly reviewed. After due consideration of the chemical structure and biological significance of glutathione, the author reviews the subject with respect to its distribution in the living organism, its variation associated with physiologic and pathologic alterations, and finally the methods of demonstrating the presence and determining the quantity of this substance in the tissues and organs. From the mass of evidence—often contradictory—which has been published, the author arrives at the somewhat discouraging opinion that due to faulty methods of determination, much of the work done so far should be repeated and that many of the earlier conclusions derived should be abandoned. He believes, however, that beyond doubt one can assume that the principal action of glutathione is that of a catalyzer in the processes of tissue autolysis and an activator of tissue enzymes. The magnitude of the task accomplished by the author will be properly appreciated by all those interested in glutathione and in the metabolic activities of sulphur compounds in general.

ÜBER KATALYSE UND KATALYSATOREN IN CHEMIE UND BIOLOGIE.

*By Alwin Mittasch. Julius Springer, Berlin.* RM. 3.60. 8½ x 5¾; vii + 65; 1936 (paper).

Although the author probably knows more about catalysts in technological processes than anyone else, and has certainly contributed more than his share of important work on the subject, it is disappointing to find how little this book contributes to the elucidation of the principles of catalytic action. No fundamental idea underlies this factual survey of the various aspects of catalysis and a great number of biological phenomena. "One cannot see the forest for the trees" aptly describes the obscurity in this field associated with the great amount of technical work that has been done in it. There is an index of names.

## SEX

PROSTITUTION. *An Investigation of Its Causes, Especially With Regard to Hereditary Factors.*

*By Tage Kemp. Levin and Munksgaard, Copenhagen; William Heinemann, London.* Dan. Cr. 12 (cloth); Dan. Cr. 10 (paper). 10 x 7½; 253; 1936.

The author has made a medico-psychiatric examination of 530 prostitutes living in Copenhagen. Since the purpose of the investigation was to study prostitution mainly from the biological standpoint, an attempt was made also to obtain as much information as possible concerning

the families of the subjects. At the time of the investigation the Danish police arrested prostitutes only when these women were found to have no regular legitimate occupation. Since the women examined were all in this category, the author believes that they are not a representative sample of prostitutes because it must be assumed that only the less intelligent ones fell into the hands of the police. This probably accounts in part for the fact that only 29.4 per cent of these women were found to be mentally normal and without defective intelligence. On medical examination, the author finds that about 50 per cent of the women had some serious chronic illness, gonorrheal salpingitis being of course the most common disease. As has been observed by others, the social and economic environment of these women is usually deplorable and there is a high incidence of alcoholism, criminality, prostitution, suicide, psychopathy, insanity and oligophrenia in their families. The fact that in some cases the mothers and maternal grandmothers were also prostitutes leads the author to believe that the mental make-up which predisposes to prostitution is frequently hereditary. While the facts, such as they are, are presented in an objective manner, the author is unable to arrive at any definite conclusion regarding the biologic aspect of prostitution. More hopeful than convinced he advocates better eugenic regulations and mental hygiene programs and believes that a reduction of social inequality might be helpful in preventing prostitution.

SEX AND PERSONALITY. *Studies in Masculinity and Femininity.*

*By Lewis M. Terman and Catharine C. Miles assisted by Jack W. Dunlap, Harold K. Edgerton, E. Lowell Kelly, Albert D. Kurtz, E. Alice McAnulty, Quinn McNemar, Maud A. Merrill, Floyd L. Ruch and Horace G. Wyatt. McGraw-Hill Book Co., New York.* $4.50. 9 x 6; xii + 600; 1936.

Terman and Miles offer in this work a masculinity-femininity test. It is a test of the mental personalities of the masculine and the feminine. The results of extensive data on this test are correlated with physical structures and psychological reactions of the individual.

Not so long ago the psychiatrist classified his subjects as "normal" or "feeble-minded," or as "normal" or "insane." Thanks largely to Binet and his successors on the one hand, and to modern psychiatry on the other, no competent investigator in abnormal psychology now regards such a simple classification as adequate or even possible. The purpose of the investigations here reported has been the accomplishment in the field of masculinity-femininity of something similar to Binet's early achievement in the field of intelligence—the quantification of procedure and of concepts.

As a pioneer effort the work deserves praise. However, the test is lengthy for convenient use and, in spite of very extensive statistical work, no particularly significant or reliable results have thus far been obtained, with the exception of its correlation with homosexuality.

PÉRIODE DE FÉCONDITÉ ET PÉRIODES DE STÉRILITÉ CHEZ LA FEMME. (*Lois d'Ogino et de Knaus.*)

*By H. Vignes and M. Robey. Masson et Cie, Paris.* 14 francs. $7\frac{7}{8}$ x $5\frac{1}{2}$; 87; 1936 (paper).

In the study of the periods of relative fecundity and sterility in the menstrual cycle two theories divide the favor of the physiologists and gynecologists. One affirms the strict limitation of the periods of physiological fecundity and sterility, while the other asserts the possibility of fecundation on each day of the menstrual cycle. This book is an impartial and detailed presentation of the respective arguments of these two schools of thought.

Vignes and Robey show how the facts in this kind of study must be interpreted, and give an example of the critical observations necessary in drawing conclusions. The observations presented here are the result of their practical experience and their intensive study of reproductive physiology. No exact law has yet been proved, and it is necessary to study each

case with individual attention and method. More delicate and exact methods must be adopted before it can be determined with assuredness whether a particular woman is the norm or the exception. The book contains a good bibliography.

WHITE WOMEN, COLOURED MEN.
*By Henry Champly. Translated from the French by Warre Bradley Wells. John Long, London.* 18s. 9 x 6; 319 + 8 plates; 1936.

The theme of this ill-organized, diffuse, and tiresomely verbose treatise is that: "*The Coloured peoples have discovered the White woman*—as a marvel; as a wonder from the physical, the artistic, the social and even the religious points of view; as an idol worthy of being desired above all else." This theme is repeated, developed, inverted, varied, and subjected to all the ornamentation of scandalous gossip that could be garnered by an ingenious author through the expedient of seeking low company in the lowest corners of cities all over the world.

In fairness it should be said that no library of pornography will be *quite* complete without this well-indexed book. But equally we should warn intending buyers that it has none of the hearty robustness of Pantagruel, but tends rather towards the slightly slimy school of the *demi-vierge*, with never a naughty word in it.

OESTRUS, RECEPTIVITY, AND MATING IN CHIMPANZEE. *Comparative Psychology Monographs, Vol. 13, Serial No. 65.*
*By Robert M. Yerkes and James H. Elder. Johns Hopkins Press, Baltimore.* 75 cents. 10 x 6¾; 39; 1936.

This is an interesting and well executed study of the reproductive cycle and copulatory behavior of the chimpanzee. It is shown for this animal that the sexual cycle lasts about five weeks and can be divided into six physiological phases—menstrual, postmenstrual, tumescent, maximal swelling, detumescent and premenstrual. The female chimpanzee largely controls the mating pattern and is highly receptive sexually during the tumescent and maximal swelling periods. This receptivity reaches its height at the time of ovulation when the female expresses her sexual desire to the male both gesturally and posturally. The authors conclude that "social and psychobiological conditions" as well as "physiological factors" are highly important in shaping the mating behavior of this primate. In this respect they feel that apes and humans have much in common.

ENCYCLOPAEDIA OF SEXUAL KNOWLEDGE.
*By A. Costler, A. Willy and others under the general editorship of Norman Haire. Eugenics Publishing Co., New York.* $6.00. 9 x 6; xx + 567; 1936.

The practiced reviewer of "sex books" could easily sit down and prepare a standardized review that would serve, with minor alterations, as an appraisal for nearly the entire group. This is another way of saying that most of these books are stereotyped and exhibit little variation or originality, regardless of author, publisher, or prospective reader. All contain remarks about the physiology of reproduction, the nature of sexual intercourse and sexual perversions, and nearly all leave the boundaries of science and enter the fields of aesthetics and ethics with poetic gestures. The present book is no exception. In terms of organization and exposition it is better than some and worse than others. In terms of content it is like all the rest. It can be recommended only with an evasive yes and no.

## BIOMETRY

ELEMENTS OF PROBABILITY.
*By H. Levy and L. Roth. Oxford University Press, New York.* $5.00. 8¾ x 5½; x + 200; 1936.

The authors have, in their own words, "striven to provide a detailed criticism of the various self-contained theories of probability that have been advanced

from time to time." The result is an excellent text, one that should be of considerable use to all students of mathematical probability.

The second chapter gives a very clear statement of the meaning of chance and of the definition of probability. The derivations of the addition and product formulae, Tchebycheff's problem, Bernoulli's theorem, etc. follow lines common to books on mathematical probability. A separate chapter is devoted to the extension of continuous distributions. The derivations of the Gaussian law and other forms of hypothetical populations are given in detail. The last chapter on "The Use of Probability in Scientific Induction" is a clear presentation of the problem of sampling. Correlation theory, and the tests of significance for small samples are treated somewhat briefly.

Numerous examples are scattered through the book. There is an index, but no bibliography.

NARRENSPIEGEL DER STATISTIK. *Die Umrisse eines statistischen Weltbildes.*

*By Ernst Wagemann. Hanseatische Verlagsanstalt, Hamburg.* RM. 6.80 (paper); RM. 7.80 (cloth). 9 x 6; viii + 255; 1935.

The remarkable nature of German official and semi-official statistics since the beginning of the Nazi regime has been the subject of not a little criticism and ridicule by distressingly skeptical foreign statisticians. The present volume is rather more of a brilliant counterattack upon the hecklers than a meek defense. They are reminded that "people in glass houses. . . "

STATISTICAL METHODS FOR RESEARCH WORKERS. *Sixth Edition, Revised and Enlarged.*

*By R. A. Fisher. Oliver and Boyd, Edinburgh and London.* 15 s. net. $8\frac{5}{8}$ x $5\frac{3}{8}$; xiii + 339 + 6 folded tables; 1936.

In this new edition of Professor Fisher's well-known and widely used text a number of additions have been made, notably a new test for homogeneity of material with hierarchical sub-divisions. The general plan and structure of the book has been in no wise altered from earlier editions.

## PSYCHOLOGY AND BEHAVIOR

THE NATURAL HISTORY OF MIND. *Tarner Lectures delivered in Trinity College, Cambridge, 1935.*

*By A. D. Ritchie. Longmans, Green and Co., New York.* $4.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; viii + 285; 1936.

This series of lectures apparently had for its major purpose the orientation of psychology in the general field of modern science and philosophy. We feel the author deserves high praise for the clear, logical, and unprejudiced manner in which he carries this work out.

We state briefly a few of the topics he develops at some length: (1) As life processes including those of the mind quite probably follow the "generic" laws of physics and chemistry, these laws will form a useful basis for psychological research but alone can never answer the highly "specific" problems of this study. (2) The ordinary conception of cause and effect may be held to be true in the macroscopic scale (in which scale psychology lies) even though a sequence of events may follow a given law only as a statistical result of random motion in the microscopic scale. (3) "The division of objects into inanimate, animate, and personal is convenient as a first approximation but cannot be the last word."

Of special interest is his critique of a number of the present day approaches to the study of psychology. The experimental method requires that one variable be changed in known quality or quantity while all other important variables be held constant. In nerve physiology this can be accomplished with single nerve fibers, but too many variables are involved in higher nervous and mental activity to make this possible. Pavlov's work with conditioned reflexes is again a study of isolated, relatively simple nervous channels rather than a study of the normal reactions of the mind. Of one such experiment the author remarks that it

was "a model of experimental technique from the point of view of eliminating uncontrolled variables; unfortunately the dog was one of the variables eliminated!"

THE SCIENTIST IN ACTION. *A Scientific Study of His Methods.*

*By William H. George. Williams and Norgate, London.* 10s. 6d. net. 8½ x 5½; 355; 1936.

To develop a complete and precise picture of the scientist at work is a difficult undertaking and requires no meager amount of analysis of both the man and his method. The author of this volume has very successfully developed just such a picture. Organized and written in a very logical and detailed manner, the book is the result of much keen and intensive scientific observation and reaction.

The fundamental idea conveyed is that scientific research is a form of human action which gives two kinds of results: the human yet impersonal observations called facts, and such arrangements of facts as classifications, laws, and theories. George believes that

> if the scientists' methods are to be applied to problems of civilization, men able to make the applications must be found and must be trained in scientific action. The scientific method itself must be taught, and more than that, it must be taught in combination with a science or some other subject in which the application is illustrated.

Stimulating in essence and progressive in outlook, the book should be a part of every research worker's private library. There is a short bibliography on the general aspects of the scientific method, and an index.

CONDUCT AND ABILITY. *A Text-book of Psychology from the Hormic and Noegenetic Standpoints, with Practical Exercises for Students.*

*By Frances Banks. Methuen and Co., London.* 10s. 6d. net. 7¼ x 4⅞; xi + 399; 1936.

This is a text book for educators. The aim of the author is to present the most recent findings in learning, remembering, forgetting, attention, etc. in a practical way for use primarily in teaching children. Part I, on the *psychology of conduct*, is devoted primarily to a discussion of the neural mechanism, reflexes, instinct and emotion from the physiological standpoint. There follow several chapters on sentiment, character, will, the development of self in relation to (a) authority, (b) society, (c) sex and (d) reality. Part II on the *psychology of ability* deals in detail with Spearman's theories of intelligence and cognition, intelligence testing (Binet-Simon), sensation, perception, learning processes, remembering and higher thought processes. Part III is devoted to *methods* of psychology and practical experiments and exercises.

Throughout the book a great deal is said about the hormic view of conduct and the noegenetic doctrine of cognition in simple language. A useful book for teachers.

PRACTICAL EXAMINATION OF PERSONALITY AND BEHAVIOR DISORDERS. *Adults and Children.*

*By Kenneth E. Appel and Edward A. Strecker. The Macmillan Co., New York.* $2.00. 8½ x 5½; xiv + 219; 1936.

This book has been designed for the use of medical students, general practitioners, psychiatrists, personnel advisors, teachers, etc. Although it is impossible to give exact rules for the practise of psychiatry, the authors believe that "psychiatry does have hints, suggestions, techniques and methods to impart" which can supply needed guidance to the beginner until he has had sufficient experience to devise methods and techniques of his own.

The first part of this book deals with the psychiatric examination of adults, the second part with the psychiatric examination of children. Both parts contain suggestions for making the examination, outlines of the information to be obtained in general and specific problems (parent-child relationships, temper tantrums, etc.), suggested tests and other practical information. The techniques described have been used by the students

in the School of Medicine at the University of Pennsylvania, by physicians in the Department of Mental and Nervous Diseases of the Pennsylvania Hospital, and in the private practise of the authors. Thus the material contained in this book has been found to fulfill its purpose not merely in theory but also in actual practise.

INTELLIGENCE.

*By Theodore Dorros.* (*This book is not for sale.*) $8\frac{3}{4}$ x $5\frac{3}{4}$; 153; 1936 (paper).

The author, as far as we can make out, believes that all human beings, everywhere, and at all times have ordered, now order, and (unless they follow his advice) will continue to order their lives with 100 per cent lack of "intelligence."

The following extract was selected at random:

> "*The only way* for the race to start on the road to intelligent collectivism is: to unceasingly reduce reproduction and coalesce its varieties. To produce units of an always 'better quality' and in an always better collective environment. It is not a question of 'supermen,' 'angels,' 'demigods,' but *simply* of eventually giving birth to 'ordinary, very ordinary mortals' with their chemistry functioning in the best possible *human* way and with an 'inherited' and actual 'environment' that would produce *most normal* conditions of collective life and physico-mental states for these *normal* mortals. Nothing else. But on the other hand *it is* a question of eliminating the 'masses' which are not needed to serve the 'superior' units, since these 'superior' ones *are not needed either*, both being *below* the first level of the elementary normal human intelligence."

[Reginald the Office Boy, whose quite normal adolescent love for rhetorical flourishes is sprouting along with his beard, says that Mr. Dorros's announcement that his book is not for sale "is a hyperbolically supererogatory contribution to the catallactics of publishing!]

THE SENSORY BASIS OF MAZE LEARNING IN RATS. *Comparative Psychology Monographs, Vol. 13, Serial No. 64.*

*By C. H. Honzik. Johns Hopkins Press, Baltimore.* $1.50. 10 x $6\frac{3}{4}$; 113; 1936 (paper).

The purpose of the investigation reported here was to study experimentally the relative influence of the different sense-organs on maze learning in rats. Twenty-three groups, each containing from 42 to 53 pigmented and albino rats, were used. The ages of the animals ranged from three to six months. Two identical elevated 14-blind mazes constructed by the author were employed in the experiment. By operative and other procedures the function of one or another sense organ was reduced or abolished. In agreement with previous investigations, the author finds that vision assumes the dominant rôle both in learning and in maintenance of the maze habit. Olfaction also plays an important part but secondary to vision. Next in importance comes audition. Tactual stimuli are of only minor importance. Kinesthesia alone is not sufficient for learning but it appears that these impulses are essential to acquisition of skill. From this series of experiments, well executed and clearly presented, the author reaches the conclusion that the motor responses constituting the maze habit are not due to specific stimuli but to complexes or patterns of stimuli. The list of references is fairly adequate.

THE PSYCHOLOGY OF DEALING WITH PEOPLE. *Appealing to the Want for a Feeling of Personal Worth.*

*By Wendell White. The Macmillan Co., New York.* $2.50. $7\frac{3}{4}$ x $5\frac{1}{2}$; xiv + 256; 1936.

Part one of this book on dealing with people in life situations in general could serve as a text for classes in elementary salesmanship, and, in fact, might serve as an introduction to the more inspirational methods of Vash Young. The remaining three parts deal with the prevention of wrong doing and otherwise peculiar behavior, and the furthering of mental health. Neat and tidy solutions are given with each sample of "peculiar" behavior. On page 217 the author quotes, with some approval from *Elementary Principles of Education* by Thorndike and Gates as follows: "When we inquire what occasions human happiness, we shall find that it depends upon human wants."

REACTIONS OF THE HUMAN MACHINE.
*By John Y. Dent. Victor Gollancz, London.* 8s. 6d. 7¾ x 5; 288; 1936.

THE HUMAN MACHINE.
*By John Y. Dent. Alfred A. Knopf, New York.* $2.50. 8⅛ x 5½; x + 294 + v; 1937.

These two volumes (one for English, the other for American readers) are unlike in format but alike in substance matter. A plea is made especially for "the physiological, material attitude of patients and doctors towards life and its discomforts and the exclusion of magic and the supernatural from the treatment of the human mechanism." An attempt is made "to describe this mechanism and the way it responds to its environment, especially the way it responds to words, to suggestions." Psychoanalysis and hypnosis are discussed in the latter part of both books. "Psychoanalysis seeks to revive buried vestiges of desire. Hypnotism seeks to bury them deeper. I consider their removal unnecessary, as they have to be buried again and this may not be easy." How to be a hypnotist is taught in one easy lesson of only six pages.

MEASUREMENT IN PSYCHOLOGY.
*By Thelma Hunt. Prentice-Hall, Inc., New York.* $3.00. 8 x 5½; xx + 471; 1936.

This text book designed for college courses in the subject completely reviews the various types of psychological measurements with a general description of the use of each. Covering so many techniques of various types in one volume, there is practically no attempt made to examine any of them critically. Unless the teacher is entirely capable of supplying this lack, the text is hardly adequate for a course intended to give a really intelligent understanding of this difficult subject. It is, however, an extremely useful book for handy reference.

## DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

SCIENTIFIC PROGRESS. *Sir Halley Stewart Lecture, 1935.*
*By Sir James Jeans, Sir William Bragg, E. V. Appleton, E. Mellanby, J. B. S. Haldane and Julian Huxley. The Macmillan Co., New York.* $2.00. 7¾ x 5½; 210 + 4 plates; 1936.

Six popular lectures are here presented by prominent British authors. The titles are, respectively: Man and the Universe; The Progress of Physical Science; The Electricity in the Atmosphere; Progress in Medical Science; Human Genetics and Human Ideals; Science and its Relation to Social Needs. Each one is excellently written and contains much material of value to biology.

In chapter IV the history of medicine is traced from its beginnings, although recent developments are emphasized—entertainingly and with profitable, rich detail. Chapter V concerns the two questions of sterilizing the unfit and racial superiority, with that usual clarity and intelligence distinguishing Professor Haldane's writings. The final chapter considers scientific research as a social-anthropological trait; its history, functions, general behavior, adaptations to environment, and dependence upon other traits, particularly those of capitalism and the profit motive. Stress is laid upon the imbalance between research motivation and social needs.

THE HARVEY LECTURES. *Delivered under the Auspices of The Harvey Society of New York 1935–1936. Under the Patronage of the New York Academy of Medicine Series XXXI.*
*By Max Bergmann, Robert M. Yerkes, Peyton Rous, B. A. Houssay, John Farquhar Fulton, Richard E. Shope, Warren H. Lewis and I. deBurgh Daly. Williams and Wilkins, Baltimore.* $4.00. 8 x 5¼; 255; 1936.

The high standard set by previous lectures in this series has been well maintained in those presented in this volume. Biologists will find The Significance of Chimpanzee-Culture for Biological Research by Robert M. Yerkes of special interest. The other lectures included in the present volume are: Proteins and proteolytic enzymes, by Max Bergmann; the virus tumors and the tumor problem, by Peyton Rous;

relations between the parathyroids, the hypophysis and the pancreas, by B. A. Houssay; the interrelation of cerebrum and cerebellum in the regulation of somatic and autonomic functions, by J. F. Fulton; the influenzas of swine and man, by Richard E. Shope; malignant cells, by Warren H. Lewis; the physiology of the bronchial vascular system, by I. de Burgh Daly. Bibliographies are provided for most of the papers. There is no index.

THE STUDY OF THE HISTORY OF SCIENCE. *By George Sarton. Harvard University Press, Cambridge.* $1.50. 8¼ x 5¾; 75; 1936.

The author defines history of science as the history of the acquisition and systemization of positive knowledge. The functioning of the historian, as he sees it, does not differ essentially from that of the entomologist. The one collects scientific ideas, the other collects insects. Both have the common objective to increase knowledge. In this small volume, Sarton emphasizes certain elementary but important points in method. Such are, for example, accuracy and precision in citing facts, and determination of the relative value of era, environment, and personalities. Included is a bibliography, almost half as long as the text matter, which contains a selected list of references that will prove very useful to the student.

THE AUTOBIOGRAPHY OF A SCIENTIST. *Being the Memoirs of Doctor Henry Manure, Professor of Archaeology, Palaeontology and Egyptology at Derbytown University as Recorded by His Amanuensis.*

*Scientific Publishing Co., Princeton.* $2.25. 9 x 6; xiv + 177; 1936.

Anybody who is anybody expects to be caricatured and usually likes it provided it does not hit a really sensitive spot. But the truth hurts. Whoever the learned "Dr. Henry Manure of Derbytown University" may be, he applies a sharp tongue and keen wit to a satire of scientists probably a little too pointed to be thoroughly enjoyed by them. Many of the points are deserving of ridicule and the book is amusing in spots, but most of the humor is rather bitter than funny. Lest others get a free laugh exclusively at the scientist's expense, the clergy, the business man, and institutions in general all come in for their share of the beating.

PREPARATION OF SCIENTIFIC AND TECHNICAL PAPERS. *Third Edition.*

*By Sam F. Trelease and Emma S. Yule. Williams and Wilkins Co., Baltimore.* $1.50. 7¼ x 5; 125; 1936.

This little book has proved its usefulness by appearing in a third edition. It has been revised and enlarged but not *sufficiently* enlarged to satisfy this reviewer. The authors give sound advice on all the main points in preparing and seeing through the press a manuscript dealing with scientific and technical subjects. But with the addition of only a few more pages—certainly not more than twenty-five—treatment of some of the more perplexing questions on punctuation, the arrangement of complicated tables, etc. would add greatly to its value.

THE HANNA STAR DOME. *Pocket Natural History No. 6, Astronomical Series No. 1. First Edition.*

*By Dorothy A. Treat. The Cleveland Museum of Natural History, Cleveland.* 25 cents. 6¾ x 3¾; 47; 1936 (paper).

The relation of this programmatic pamphlet to biology may seem to the superficial mind a bit on the exiguous side, but such a view overlooks some important points, to wit: (1) Mark Hanna was, after all, a quite extraordinary specimen of the genus *Homo*; (2) as scientific amusement enterprises planetaria are running close seconds in gate receipts to the anatomical

exhibitions sponsored by the Minsky Frères; (3) natural history museums acting as planetarium entrepreneurs get the rake-off; and so (4) are by that much better able to promote their strictly biological activities. Thus all works together *ad majorem gloriam Dei*, and merits—even demands—notice in this forward-looking Family Magazine for Biological Homes.

UNIVERSITY OF COLORADO STUDIES, *Volume 24, Number 1. Abstracts of Theses and Reports for Higher Degrees.*
*University of Colorado Press, Boulder.* $1.00. 10 x 6⅞; 77; 1936 (paper).

TUFTS COLLEGE STUDIES, *Volume VI, No. I.* (*Scientific Series Nos. 51–56.*)
*Tufts College Library, Tufts College, Mass.* 9 x 6; 72; 1936 (paper).

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